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PATH OF MARS FROM 1875 to 1892.

BY a mistake, for which I fear I have no one but myself to thank, 1877 was printed for 1892 in the heading to the looped path of Mars from 1875 to 1892. As there are some 600 positions of the planet (all separately laid down before the path was carried through them) and the constructions for these involved many hours of labour, it was rather annoying to find the diagram appearing as if it only showed the planet's path during two or three past years, instead of showing it for seventeen years, ten of which have still to pass. For a time I felt disposed to reject Liebig's saying that "there is no harm in making a mistake."—ED.

THE ANTIQUITY OF MAN IN WESTERN EUROPE.

BY EDWIN CLODD.

IT is well known that the period from the unknown date of man's appearance in Europe until about the Christian era, is divided by antiquarians into the Ages of Stone, Bronze, and Iron. Such a division, anticipated nearly 2,000 years ago by Lucretius in his immortal poem, *De Rerum Naturâ*,* is not restricted to one quarter of the globe, but holds good for every part habitable once or inhabited now; a mass of ever-increasing evidence being producible to show that the use of stone and other accessible and pliable materials preceded that discovery of metals which placed so powerful an instrument of control and advancement in the hands of mankind. These divisions of stone and metal-using periods, it should be borne in mind, are not to be applied to all parts of the world at one and the same time, as if there had been a universal and co-temporaneous abolition of stone at a given epoch in human history, and a universal adoption of the compound metal bronze in its place. In the later periods of the Stone Age, it is certain that Europe was occupied by races in markedly varying degrees of civilisation. The people settled along the shores of the Mediterranean were far ahead of those scattered over Northern Europe, iron being probably known to the former, while the latter still used ground and polished stone implements, or bartered the much-prized amber for Etruskan and Phœnician bronze. So in the present day, widely as metals are dispersed by traders, we find barbarous races who still make shift with tools and weapons of stone.

But it is more important for our present purpose to point out what is not so well known, namely, that the subdivision of the Stone Age into the Palæolithic or Older, and the Neolithic or Newer, marks a difference between these, which is in every respect greater than that between the Neolithic and succeeding ages. Whilst these latter cover a comparatively trifling, although crowded, span of man's tenancy of Europe, and one over which the line of his advance is, if dim and zigzag, unbroken; the Palæolithic is of remote origin, of unknown, but certainly vast, duration, and but sparsely marked with the traces of his presence. The men of Neolithic times, concerning whom Mr. Grant Allen has given the readers of KNOWLEDGE a vivid and accurate sketch, are the direct ancestors of peoples of whom remnants yet lurk in out-of-the-way corners of Europe, where they have been squeezed or stranded; but the men of Palæolithic times can be identified with no existing races; they were savages of a more degraded type than any extant; tall, yet barely erect, with short legs and twisted knees, with prognathous—that is, projecting, ape-like jaws, and small brains. Whence they came we cannot tell, and their "grave knoweth no man to this day." The implements of the ancient Stone Age, mainly of flint, sometimes of chert (an impure, flint-like quartz), coarse, rough-chipped and unpolished, can never be confounded with those of the later age, which are made of divers native or imported materials ground to a fine edge and polished, often exquisitely shaped, and highly ornamented. Whilst those of Neolithic times are found in surface remains, cavern-floors, camps, temples, tombs, and mounds, from stately tumuli to rubbish-heaps on the Baltic coasts, all more or less within the province of the antiquary, those of Palæolithic times are unearthed by the geologist from ancient river-valleys, from "caves and dens of the earth," and from deposits so venerable that their contents demand a far higher antiquity for man than many anthropologists, by no means the slaves of Archbishop Ussher's chronology, are as yet willing to allow. Between the Older and Newer Stone Ages there is fixed the great gulf of climatal change and of altered distribution in land and water, for while the early, if not the earliest, Neolithic immigrants into Britain traversed a continent which, certain northern parts excepted, has undergone only local changes since they crossed it on their westward path, Palæolithic man passed without hindrance where now flows the English Channel; the mainland stretched northwards beyond Ireland and Iceland; through a forest-covered area over which the German Ocean rolls ran the Rhine, its waters swelled by streams now known as the Thames and Humber, to empty itself in the North Atlantic. Southward, the continent was joined to Africa at Morocco and Tunis, dividing the Mediterranean into land-locked seas, and making easy passage for man and brute from tropical to cooler zones. Whilst the animals brought by Neolithic races from the East were, in the main, those familiar to ourselves, those with which Palæolithic man waged war in forest and swamp were mammals now wholly extinct, or found only in arctic or tropical latitudes. The curious admixture with human relics of remains of animals adapted to widely different regions

*Bk. v., 1,282. Munro's tr., p. 268.

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has raised some interesting questions concerning the causes and duration of changes in climate which permitted, now northern species (as the mammoth or woolly-haired elephant, the musk sheep, the reindeer, etc.); now southern species (as the hippopotamus, hyæna, lion, etc.), to roam over the same areas, finding at last a common grave. But to discuss this at present would be to digress.

The above outline of the leading contrasts between the two Stone Ages may suffice to show that our subject lies far beyond the historic horizon. Ordinary landmarks and methods of reckoning, therefore, failing us, we can have some idea of man's place in geological time only by ascertaining the relative position of those deposits in which traces of him are believed to occur to the general system of organic-bearing rocks. Such traces are in truth indicated by what man has done, rather than by himself, for of him scanty are the relics—only a jawbone or fibula (small leg-bone) here, only a skull-fragment there, a paucity for which we must hereafter seek an explanation.

Vast as is the period in this world's history since the appearance of man even in Europe, it is but a fraction compared with that which extends from the beginning of life upon the earth to Palæolithic times. Hæckel* remarks that if we divide that period into a hundred equal parts, and then, corresponding to the thickness of the systems of strata, calculate the relative duration of the five main divisions or periods according to percentages, we obtain the following result:—

Primordial Epoch ..... (70,000 feet.)	Laurentian System Cambrian Silurian	53·6
Primary..... (42,000 feet.)	Devonian ..... Coal Permian	32·1
Secondary ..... (15,000 feet.)	Trias..... ..... Jura Chalk	11·5
Tertiary..... (3,000 feet.)	Eocene ..... Miocene Pliocene	2·3
Quaternary (or Pleistocene) (500 or 700 feet.) †	Palæolithic Man ... Neolithic and Historical Period	0·5

Now, it would be in defiance of all that the doctrine of evolution teaches, and, moreover, win no support from believers in special creation and the fixity of species, to seek for so highly specialised a mammalian as man at an early stage in the life-history of the globe. Even in the Secondary epoch, the only mammals which have been discovered in Europe are the fossil remains of a small marsupial or pouch-bearer; and although the placental mammals and the order of Primates, to which man is related, appear in Tertiary times, and the climate, tropical in the Eocene age, warm in the Miocene, and temperate in the Pliocene, was favourable to his presence, the proofs of his existence in Europe before the close of the Tertiary epoch, although considered sufficient by many foreign *savans*, are not generally accepted here.

It is at this point, however, that the interest of the matter deepens. No anthropologist of repute denies the *à priori* probability of man's presence in Europe under the favourable conditions of mid-Tertiary times; and the remembrance of what incredulity met the discovery in 1847 of relics indisputably of human origin in hitherto undisturbed deposits in the Somme Valley, checks incautious and hasty treatment. Let us glance at the evidence on which the French advocates of Miocene man now rest their case. Recognising the enormous duration of the Palæolithic Age, one of the most eminent of their number, M. Gabriel de Mortillet, has divided it into several well-marked stages, certainly with such warranty as all that we have at hand in support of the slow rate of human advance gives. Of these divisions, five in number, M. de Mortillet places the earliest in the mid-Miocene period.

* "Hist. of Creation," vol. ii. p. 20.

† The thicknesses of this and the other deposits are only approximately true.

FOUND LINKS.

By Dr. Andrew Wilson, F.R.S.E., F.L.S.

PART IV.

THERE are no two classes of animals between which exists a greater dissimilarity than birds and reptiles. The active organisation of the one and the sluggish ways of the other, the warm blood of the former and the cold blood of the latter, are points in the popular natural history of the two groups which technical zoology has but emphasised in its turn. Yet the scientific examination of these beings reveals bonds of connection between them, all unsuspected by the ordinary reader, and demonstrates further, in the most suggestive fashion, that the likenesses to be presently alluded to must possess some origin and meaning. That origin, evolution maintains, is "descent" from a common stock; the meaning is that seen throughout all similar series of likenesses, namely, the natural result of the laws of animal development. In the case of birds and reptiles, the same considerations appeal to us which I have already indicated as existent in the details of frog-development. Either the likenesses science discovers between apparently distinct groups of animals are explicable, or they are not explicable. If the former, then science declares, with unanimous voice, that the likenesses are due to common descent, as the unlikenesses are due to the variations and modifications produced during the evolution of the race. If, on the other hand, the likenesses are inexplicable—as I hold them to be on any other theory save that of evolution—then must mankind fold their hands in the acknowledgment of an ignorance that might legitimately, by its avowal, close the door to astronomical research, to geological work, and to scientific investigation of every kind.

I am led to make these remarks because several correspondents have remarked to the editor, that because likenesses can be proved to exist between two different groups of animals in their young state, they do not understand why the evolutionist should lay such stress upon these facts as proving his contentions. One correspondent, for instance, says that he cannot admit that because one thing is like another, the two things must stand in the relation of parent and offspring. I reply, likeness does not necessarily imply similarity of origin, but, on the other hand, it is one of the proofs of such similarity. If likeness is to be denied its place as a proof of common origin—apart from other and equally powerful proofs known to biologists—what guarantee should we possess that unlikeness means dissimilarity? That the likeness of child to parent is a natural likeness, every one must admit. The reasons are clear enough, and they derive their force from the fact that the latter begets the former. I hold that the likenesses existent—especially in the *early* stages of development—between different groups, are to be judged on the same basis, namely that of *heredity*. A manifest resemblance in the young frog to a fish is, I repeat, inexplicable, equally on scientific principles and on common sense grounds, unless on the hypothesis that some bond of relationship connects the two. The duty of

disproving this idea rests with those who deny evolution. Until we receive a fuller and more likely explanation of such likenesses as those we are at present discussing, we are entitled to hold to the only theory, which, so far as I know, satisfies the requirements of a good hypothesis—these requirements being that *it explains all the facts and is contrary to none.* This end the theory of evolution attains in explaining both the likenesses and the dissimilarities of living nature.

Returning, after this needful digression, to the case of birds and reptiles, let us firstly note the structural points in which these classes agree. To begin with, the skull of both is joined to the spine by one bony process or *condyle.* There are two of these processes in frogs and their neighbours, and a similar number in quadrupeds, including man. Then, secondly, the lower jaw of a reptile agrees with that of a bird in its compound nature. This jaw, instead of being simple and composed of two simple halves (as in quadrupeds), consists in birds and reptiles of from eight to twelve distinct pieces, which are amalgamated to form one bone. Furthermore, whilst the quadruped's lower jaw is joined directly, and of itself, to the skull, that of the bird and reptile is attached to the skull through the medium of a distinct bone, which is named the *quadrate bone.* Curiously enough, this bone in the quadruped is pushed upwards into the middle of the skull in the course of development, and becomes one of the three small bones (*malleus*) of the internal ear. Again, reptiles and birds agree in possessing lungs alone as their breathing organs. No gills are developed (as in frogs and fishes) at any period of reptile or bird-life, although both, like quadrupeds, possess *gill-clefts* in the neck in early life. These "gill-clefts," seen in the early life of man himself, are to be viewed as feeble survivals of the aquatic ancestry from which, according to evolution, all Vertebrate animals have sprung. Furthermore, instead of the ankle-joint (as in man and quadrupeds) being situated between the end of the leg, so to speak, and the beginning of the ankle-bones, this joint in reptiles and birds exists in the middle of the ankle-bones themselves. This curious feature will be further alluded to later on.

The technical naturalist would enumerate other points of agreement between birds and reptiles, but sufficient has been said to show the close affinities which lie just beneath the surface of their organisation. Their differences, however, are also of pronounced type. The causes to which in the far-back past the evolutionist conceives the likeness between these animals to be due, have operated, through variation, at a less remote period, to produce the divergent lines of development. Thus we discover that birds are warm-blooded, whilst reptiles possess cold blood; the bird's feathers are unknown in the reptile-world; and the perfect heart and circulation of the bird—similar to that of man—are also unrepresented in reptiles. Crocodiles, which possess a four-chambered heart, like birds and quadrupeds, nevertheless exhibit the same imperfect and "mixed" circulation seen equally in frogs and reptiles. The lungs of birds are of "open" structure, and part of the air inspired passes through the lungs to fill certain "air sacs" in the bird's body, and also fills the interior of the bones in most birds. Such a distribution of air in the bird's body is evidently adapted for the exigencies of flight. On the whole, then, with certain well-marked likenesses—which, be it observed, evolution accounts for on the idea of a common origin—the classes of birds and reptiles are demarcated from one another by certain highly-distinctive characters.

The dissimilarities on the hypothesis of evolution are due to variation and modification; but, if this idea be correct, can we show the stages through which the variation has led these two groups? In other words, have the "links" which should hypothetically connect them, any existence whatever? Such an inquiry would have been answered in the negative only a few years ago; but, thanks to recent research, we are now enabled, satisfactorily enough, to bridge the gulf between birds and reptiles, and in a measure to reconstruct the pedigree of these curious races.

To render my remarks clear, it may be well at this stage to show in a tabular form the relative positions of the rock-formations with which we shall have to deal. Placed in the order in which they occur in the earth's crust, the rocks in question lie thus:—

Tertiary Rocks.	Recent.
	Pliocene.
	Miocene.
	Eocene.
Secondary Rocks.	Chalk.
	Oolite.
	Trias.

The meaning of this table becomes clear, if it be borne in mind that the rocks as here noted are divided into the older Secondaries and the newer Tertiaries. The Eocene in turn is the oldest (or lowest) series of the Tertiary rocks, as the Trias is the oldest of the Secondary rocks.

The fossil remains of birds are few and far between, and this for the reason pointed out by Lyell—namely, that the body of a bird falling into water, prior to its entombment in the deposits which form the rocks of the future, would float, and would afford a likely object of prey to other animals; thus escaping the chances of preservation. For long, fossil birds were regarded as limited to the Tertiary rocks; but we now know of their existence in the Chalk, or Cretaceous Period; and we have also obtained fossil specimens from the rocks immediately preceding the Chalk in time, namely the Oolitic or Jurassic Period.

It is almost needless to remark that the bird-remains of the Tertiary rocks, as a rule, resemble closely the birds of our own day. In this light they only testify to the age of some of our existing groups of birds, and do not directly support the theory of evolution, whilst, of course, they do not in any way negative it. But in the deposits of the London clay of Sheppey, belonging to the Eocene (Tertiary) period of geology, the remains of a bird, belonging apparently to the swimmers, were discovered. This form was named *Odontopteryx* by Professor Owen, and its remarkable jaw-armature at once attracted the notice of naturalists. No existing bird has teeth; and no bird possesses any structures approaching teeth in function—save, perhaps, such birds as the Mergansers, in which the horny margin of the jaw is cut into a series of projections, adapted for retaining a secure hold of the finny prey on which these birds subsist. But in the *Odontopteryx*, the jaws were beset with strong bony processes, which, though resembling teeth in appearance, nevertheless are mere projections of bone—for, as most readers know, teeth are not of bony nature, but possess a special and distinct structure of their own. Nevertheless, the fact of this extinct bird of the Eocene rocks possessing toothed projections of its jaws, serves to link it, in the opinion of naturalists, to the reptile hosts; for teeth are as stable and characteristic possessions of the reptile class as their absence is a natural feature of existing birds.

(To be continued.)

The Sound, or Swim-bladder of Fish.—*Erratum.* In my letter (Knowledge, March 17, p. 439), for "a swimming pair," read "a swimming *paw*"; for "straight," read "strait."—W. Houghton.

PHOTOGRAPHY FOR AMATEURS.

By A. Brothers, F.R.A.S.

PART II.

FEW subjects connected with science present more points of interest than the discovery of photography. M. Niepce, as we have seen, laboured most patiently for many years, and only achieved partial success. Independently, Daguerre was working with a similar object—that of fixing the image produced by light when projected by means of a lens on to a sensitive chemical compound. Also independently, Mr. H. Fox-Talbot commenced experiments for fixing the photographic image. In his work, "The Pencil of Nature," Talbot relates that in 1834, while sketching the scenery of Lake Como by means of the camera lucida, the idea occurred to him that it ought to be possible to fix the image produced by light by chemical means; and on his return to England he commenced researches, and continued them with such success, that in 1839 he read a paper before the Royal Society announcing his discovery, and in 1841 he secured his process by patent. This patent, however, he very generously resigned a few years later.

In his "History and Handbook of Photography," Tissandier relates a circumstance which I have not seen elsewhere, and it deserves to be repeated, as it shows that others besides those savants already named were busy trying to solve the difficult problem of painting by sunlight. The authority quoted is "Guide du Photographie," Paris, 1856, and the narrator of the fact is M. Chevalier, an optician. Daguerre and others were in the habit of frequenting Chevalier's shop, and in 1825 a young man called to inquire the price of a new lens which he had heard Chevalier was making. The price appearing to be beyond the man's means, he was asked for what purpose he wanted the lens, when he said that he had succeeded in fixing the light-produced image. Chevalier thought "Here is another of those poor fools who want to fix the image of the camera obscura!" But Chevalier was astonished when the man placed a paper on the counter. "That," he said, "is what I can obtain." Chevalier looked at it, and was astonished to find a view of Paris as sharp as the image of the camera. The inventor had fixed the view of Paris as seen from his window.

Here was an instance of success achieved, but for want of means, the discovery could not be carried further, and when we remember the importance of the matter, it is strange that the discoverer is never heard of again.

It is unlikely that Talbot could have known what others were doing, as it was seen, when the two processes were compared, that they were totally different. Daguerre's pictures were on polished silver, made sensitive with the fumes of iodine. This image was *latent;* that is, nothing could be seen until the vapour of mercury was caused to *develope* the image; that is, the mercury was deposited in proportion to the action of the light on the plate. Talbot's process was altogether different. The image was produced on paper, and was a *negative.* The advantage over the Daguerreotype in this case was that an almost unlimited number of copies could be produced as *positives;* that is, prints having the lights and shades as in nature. Both of these valuable discoveries were to some extent due to accidents. Daguerre noticed the effect produced on a plate coated with iodine, on which an object had been accidentally placed; and Talbot observed that gallic acid, which had been spilt on his prepared paper after exposure to light, developed the latent image.

These are the main facts relating to the discovery of the photographic art, and further detail here is unnecessary. Both of the processes named were extremely slow, several minutes being necessary to produce a portrait. But improvements were rapidly made. After bromine was introduced into the process by the late Mr. John Goddard, the Daguerreotype plates were so sensitive to light, that a rapidly-revolving wheel was photographed while illuminated by an electric spark—a feat which can scarcely be surpassed in the present day.

Many new, and modifications of old, processes were introduced, but it remained for the late Mr. F. Scott Archer to simplify and improve the method of obtaining the image formed by light—the great improvement being the use of glass to bear the film on which the image was to be fixed. Various attempts had been made by the late Sir John Herschel and others to utilise glass, but it was Mr. Archer who succeeded with *collodion*—a fluid substance, formed of gun-cotton dissolved in ether and alcohol. Archer did not *discover* collodion, but used it as a vehicle to hold iodides and bromides which were made sensitive to light in the way we shall see in a future paper.

NOTES ON ROWING.

By an Old Club Captain.

WHAT sort of stroke is necessary properly to drive at racing speed the modern racing boat, is sufficiently clear, as we have shown. How that stroke is to be given remains to be considered.

I take it, there can be no room for doubt on one point. The old-fashioned rule, according to which the arms acted merely as stretchers in the first part of the stroke—in fact, until the body became upright—can no longer be right. A stroke beginning under those conditions must necessarily be sluggish at the beginning, and want the sharp grip or catch which we have seen to be absolutely essential. On the other hand, arm work alone at the beginning of the stroke is utterly bad. But the stroke is taken with such sharpness, to give the sledge-hammer blow necessary for propulsion in the modern racing craft, that it is by no means easy, even for the *possessor* of a perfect—that is, of the most effective—rowing style, to say exactly how his work is done. Mr. Muybridge's photographs of rapid movements by athletes have shown that men who can accomplish some feat in gymnastics, and even champions in such ordinary actions as running and jumping, often have (Mr. Muybridge, when I was talking to him on the subject at San Francisco, said they *always* have) the most incorrect ideas as to the way in which they perform their feats. Accordingly, we must not take it too surely for granted that because an oarsman says he does this or that in taking his stroke, he necessarily does so. Rapid photographs alone could tell us precisely how the best oarsmen take the stroke; but, for my own part, I feel assured, as well from practical as from theoretical considerations, that while the body is moving from its most forward position to uprightness, the arms do so much of their work that they are very considerably bent by the time the body is upright. Many Oxford men say, and doubtless believe, that in the Oxford style the arms are not bent at all till the body begins to pass the upright position. All Cambridge men know that at Cambridge the old rule used to be regarded as absolutely to be observed, that till the body was upright the arms should do no work. At Cambridge the rule was rigidly followed. In the Oxford style I feel satisfied, from repeated observations, it is not followed; and not a few Oxford men have, on carefully studying the matter, admitted to me that this is the case.

Of course, I do not advocate the undue use of the arms at the beginning of the stroke. To say truth, I in a sense advocate nothing except the intensely energetic stroke, which we have seen is absolutely essential to swift propulsion in a racing craft of the lighter sort. That such a stroke must be taken is certain. The efforts to be made in taking it are scarcely matter for theorising. Nature solves the question for herself. Where the good and the bad styles differ is not *here*, but in the choice of the kind of stroke to be given. The man who sets himself to row a long, sluggish stroke in the water, with a very rapid recovery, will row in bad style, because the kind of stroke he sets himself to row is inherently bad. The oarsman, on the other hand, who decides to take his oar through the water with all the energy he can bring to bear on the work, using the recovery to gather his energies for the next great effort, will row in good style (assuming always that he has mastered elementary matters in rowing), because *that* is the kind of stroke which ought to be taken, and there is only one way in which it can be taken.

Albeit, as the learner may be helped to acquire a good style (we speak always, be it remembered, of a racing boat at racing speed) by being told how to take his stroke, almost as well as by being told what sort of stroke to take, it may be useful to note that if at the very beginning of the stroke the muscles of the arm are all, as it were, tautened, so that the arm feels the work actively—not merely passively—from the beginning, the stroke will be begun under much more favourable conditions than if the arms be regarded merely as stretchers to bear the strain resulting from the action of the legs. There must be no jerking, no arm work done alone, but from the beginning the arms must assist the body and the legs. The other way of rowing, however suitable for the old-fashioned boats and for tub practice, is as unsuitable where a sledge-hammer stroke (long but full of energy) has to be taken, as would be the alternate action of the two arms. [And in passing, I may note that quite a large proportion of oarsmen who consider they know something of rowing, fail to use both arms with equal vigour, either in each stroke, regarded as a whole, or in the different parts of one and the same stroke. With many oarsmen the outside arm (that is the arm furthest from the rowlocks) does more than its fair share of work at the beginning and less than its fair share at the end of each stroke. It should hardly be necessary to say that this is a very bad fault indeed.] The great difference between the old-fashioned and the modern style, as regards the way in which the work is done, consists in this: that whereas in the old-fashioned style the best way to bring arms, legs, and back into effective co-operation was to let the stress of the work fall on these at slightly different times, in the modern style the best way is to bring all these forces into action simultaneously.

But the effort thus made, though it lasts a shorter time, is more exhausting than in the old-fashioned style. It cannot possibly be followed by the old-fashioned lightning feather. There must be a recovery of energy as well as a recovery of the oar. Hence the necessity for but a moderately quick recovery, and for a momentary pause just before the beginning of the next stroke.

The oar being a shorter time in the water, and only the recovery somewhat slower than in the old-fashioned stroke, it might seem as though more strokes could be taken per minute than in the old-fashioned boats, or at any rate, the effort be less; but as a matter of fact this is not the case. In the old "Hints on Rowing by Oarsmen," a stroke of 44 or 46 to the minute is spoken of as suitable for racing speed. This rate could not possibly be maintained now, even during a spurt. From 36 to 40 strokes per minute is about the quickest rate obtained by the best oarsmen of the day, and some races have been won, and well won, with a stroke never exceeding 35 to the minute. Again, as every oarsman who has tried both well knows, it is much more exhausting to row at racing speed in a light boat than in the comparatively heavy racing boat of former days. (I believe that, regarded as exercise, the older style of rowing is the better; but the object of racing is not exercise, but to attain the greatest possible speed).

There are some who, despite the absolute demonstration which, as we have seen, can be given to the proposition that with our modern racing-boat the oar *must* be a shorter time in the water than with the old-fashioned boats, insist on reiterating their belief in the necessity for long, dragging strokes,* and a very rapid recovery. For them, I venture to quote some very sensible and apt remarks by "Wat Bradwood." They were written at a time (1868) when Cambridge exaggerated as far as possible the faults (for modern racing-boats) of the old-fashioned style, and when Oxford had carried the later style almost to perfection. Observing from the Umpire's boat, he says:—"The styles of progress of the two boats themselves are palpably distinct: Cambridge take a shorter time to come through the air than to row through the water; they go much farther backward than Oxford, and are very slow in getting the hands off the chest; their boat is drawn through the water at each stroke, but has hardly any perceptible 'lift.' Oxford, on the other hand, are just the reverse of Cambridge, a long time in getting forward, and very fast through the water, 'driving the oar through' (really against the water) 'with a hit like sledge-hammers.' The general style of Oxford has not deteriorated; though many outsiders fancied that Oxford rowed a shorter stroke, it was more that the time occupied by them in slashing the oar through the water was short than the reach itself; this deceived inexperienced eyes, especially when compared with the slow 'drag through' of Cambridge, which often appeared, for similar reasons, a longer stroke than it really was."

And here let me make a few remarks on the utterly absurd criticisms of racing crews made in many of the daily and weekly journals. It would be interesting to inquire how the writers of some of these criticisms have put their notes together. I fancy their plan must be to talk with half-a-dozen watermen who have seen the boats at practice (or *say* they have), and to "average" what they get from these authorities. At any rate, the criticisms are often ludicrous in the extreme. To begin with, it seems *en règle* to say every year of the University crews that they are far below the average. I have seen this repeated *every year since* 1856, so that as in some years we have been told that the best of the two crews would not have a show with a "second eight" of any London Club, the University crew of this year ought to be a very bad one indeed. Some of these clever critics ought to man an eight, and after getting into due training and practice, take her on the top of a good tide from Putney to Mortlake, in less than $20\frac{1}{2}$ minutes, as the Oxford crew did this year in their first trial (under 20 minutes at the second trial, but rowing down stream); or else learn, when they had taken nearer 30 minutes in covering the course, to describe the Oxford

* "Pendragon," in the *Referee*, three years since, fell into this very natural mistake—especially natural in his own case, knowing, as he does, much more from personal experience about sprint running than about rowing. It requires practice both in racing craft and in so-called tubs (really very much lighter boats than those in which most people have ever tried rowing) to understand the difference between a sharp stroke and a quick stroke, between a long-lasting stroke and a stroke long in the water.

XUM

and Cambridge crews as something better than "inefficient mediocrities." Then again, to hear these critics, one might well believe that the eight picked oarsmen of either University hardly knew how to get their oars out of the water without crab-catching. The way in which they mistake cause and effect is amusing. They watch the Cambridge or Oxford boat rowing with a strong wind on the starboard beam, so that she is down on the stroke side for awhile; and we hear the terrible news that, stroke and six, or perhaps all the stroke side, "row too deep, the consequence being that they bring the boat down on their side." The height of absurdity, however, was attained last week in the *Referee*, which assuming the inferiority, or rather the utter worthlessness, of both the University crews (of "inefficient mediocrities") this year, gravely hinted that it had been attributed to favouritism in the selection of the men to form the crews. This well-informed sporting paper hopes the report may not be true, but considers that if it is untrue, it should be contradicted: so that Tom and 'Arry, after reading the paragraph, are left under the pleasing impression that "them nobs" can "nobble a race" as thoroughly as the worst "dark rowers" on the river. If Pendragon had ever attended Captains' meetings at either University, he would know how pleasant would be the fate in store for a "President" who should make up a University crew of inefficient mediocrities, or from any but the picked oarsmen of the best eights on the river. That on some occasions there may have been one or two men out of the "eight" who would have done a shade better than one or two in the 'Varsity crew, may be admitted: but readers of the *Referee* may rest assured that the President of either University Club selects *always*, to the best of his judgment, the eight men he believes to be fittest.

THE SUN IN APRIL.

[Notice.—The following notes are prepared on the supposition that those who will read them are provided with some almanac in which the astronomical phenomena are announced. The advanced student of astronomy usually has the *Nautical Almanac*, but for the amateur, Whitaker's Almanac will be found amply sufficient. Many other almanacs, though not so specially adapted to the requirements of the amateur astronomer, contain much astronomical information; and scarcely any fail to indicate such matters as the epochs of the lunar phases; the hours of rising and setting of the sun, moon, and planets; the equation of time, and so on. It has therefore not seemed desirable to occupy space with such details, but rather to provide matter which might prove of real assistance to the student.]

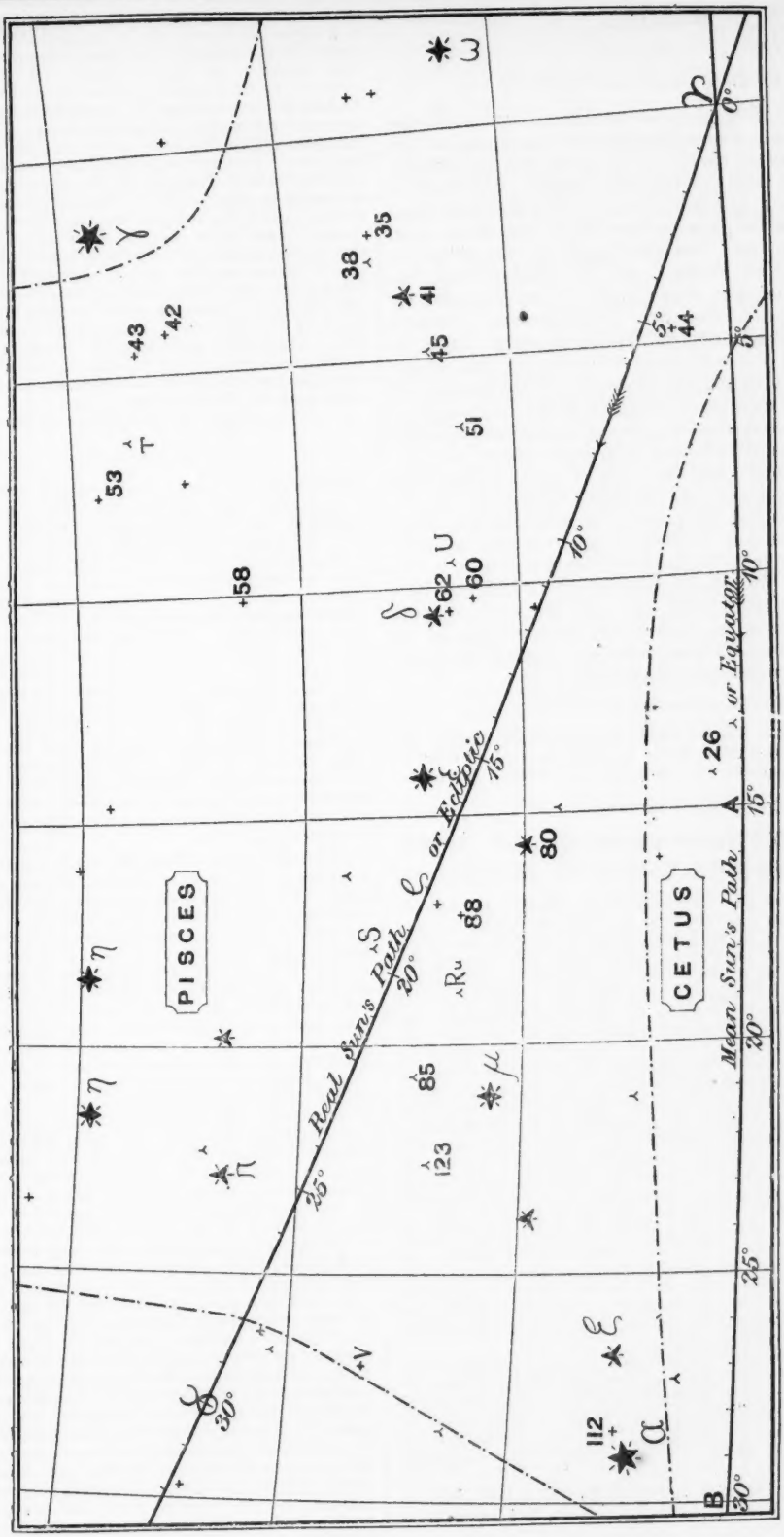
THE sun's path during the month of April, or rather from March 20 to April 20, is shown in the accompanying map from ♈ to ♉; that is, from the place where the sun's centre crosses the equator ascendingly, or northwards, through the part of the ecliptic corresponding to the sign Aries. On March 20, at 11 a.m., the sun passed the point called the First Point of Aries, and spring commenced. His rate of motion at this time does not differ much from his mean rate, and in the course of his passage through the first sign, Aries, he passes one of the points of his annual course where he is at his mean distance and moves with his mean rate of motion. (This occurs on April 1, and again after the autumnal equinox on October 3.) But although during March and April the sun moves at nearly his mean rate along the ecliptic, his path is at its greatest inclination to the equator. Thus, while he travels along 30 degrees of the ecliptic, reaching the point ♉, he has not advanced 30 degrees in right ascension, which is measured parallel to the equator. We see that the point ♉ instead of being in R. A. 2h., or 30°, he is only in R. A. 1h. 50m., or 57½° (see the illustrative map). The mean sun, therefore, which is supposed to travel at a uniform rate along the equator, gains during March and April on the real sun. But we must not fall here into the mistake made in some elementary treatises on astronomy (Mr. Lockyer's, for instance), where it is said that *therefore* the clock gains on the sun. The sun's loss in right ascension acts the other way. For as he moves annually in a direction contrary to that of the diurnal rotation of the heavens, the more slowly he moves in right ascension the shorter distance has he to make up in coming to the meridian. The eastwardly motion of the mean sun (from right to left in our map) causes the difference of nearly four minutes between a mean solar or civil day, and the sidereal day. The smaller amount of the real sun's eastwardly motion, measured parallel to the equator, causes the real solar day in March and April to exceed the sidereal day in less degree. Solar noon occurs sooner than it would if the real sun moved like the mean sun. Accordingly, whereas on March 20, at noon, the clock was nearly 7½ min. faster than the sun, this amount has been gradually diminishing day by day since then; it will be less than 4 min. on the day when this number of Knowledge is published, little more than 2 min. on Thursday, April 7, 11·51 sec. on April 14; while at noon on April 15 the sun will be 3·47 sec. before the clock, and about 1 min. before the clock when the sun completes his passage through the sign Aries.

It is to these changes that the seeming irregularity of the day, as measured from sunrise to sunset, by the clock, is due. If the time of solar noon corresponded to noon by the clock, the hour of sunrise would be almost exactly as many hours and minutes *before* as the hour of sunset would be *after* the time of noon. But when the hour of solar noon follows the hour of clock noon, the time of sunrise comes so many minutes nearer to clock noon, while the time of sunset is as many minutes farther from clock noon; in other words, the sun seems both to rise later and to set later than it does by solar or natural time. Thus, on March 20 the sun rises at 6 h. 5 m., only 5 h. 55 m. before noon, and sets at 6 h. 10 m., or by so long (15 m. longer) after noon.* Whereas, on April 18th, when the equation of time vanishes, or the real sun and the mean sun are together (in right ascension), the sun rises at 5 h. 6 m., and sets at 6 h. 54 m., in each case 6 h. 54 m. from noon.

Asthma and Tobacco.—In reply to J. W. Brookes, I beg to state that tobacco is sometimes recommended to persons suffering from asthma, on account of its influence in deadening temporarily those nerves upon whose irritation the asthmatic spasm depends. Trousseau, an eminent French physician, who suffered from this complaint, mentions that "it often sufficed for him to take a few whiffs from a cigar to free himself from his asthmatic trouble." And, on the other hand, he says that he got an attack of asthma whenever he remained in a room in which there was a bouquet of violets. The aroma from the violets acted as a stimulus to the nasal nerve-twigs, which stimulus was communicated to the nerves governing the bronchial tubes, and thus contraction of those tubes, and consequent difficulty of breathing, were produced. Tobacco, on the other hand, causes temporary paralysis of the same nerves, and renders them incapable of producing contraction of the air-tubes. But its effects are merely palliative, not curative.—J. Muir Howie.

* Note that on this day, the sun, being on the equator, would rise at six and set at six were it not for refraction (almost exactly, but the progression of the sun across the equator causes a slight difference, the sun being south of the equator when rising, and north of the equator when setting). But refraction, by raising him into view before he has really crossed the plane of the horizon, and keeping him still in view after he has really passed over it, makes the day last rather longer than twelve hours on the day of the equinox.

THE SUN IN APRIL.



Reviews.

THE TWO HEMISPHERES.*

MR. CHISHOLM gives as complete an account of the continents and countries, the oceans and seas, of the two hemispheres, as could well be presented in a single volume (but of nearly a thousand pages). The book is pleasantly written, and the information for the most part accurate, though in places rather behind the time. For instance, we are told that the largest of the rivers in North Island, New Zealand, are the Waikato and the Manawatu, no mention being made of the Wanganui, a much more important river than the Manawatu. Some of the information respecting the United States is singularly out of date, and serves to give but inadequate ideas of the progress made in the Western States during the last quarter of a century, in some cases even within the last ten years or so, though statistical information is in most cases brought up to date. The illustrations of natural scenery are, for the most part, good; but the views of towns are in many cases behind the time. A "street in Chicago" gives no correct idea of any of the principal streets in that wonderful city; and the view of Madison-square must have been taken before the gigantic Hand of Liberty had been set up which has now for several years formed a characteristic feature of the square. Still, these are not points of great importance; and in the main we are struck rather by the amount of exact and recent information which the book contains, than by occasional shortcomings.

Messrs. Blackie & Son note that Messrs. Marcus Ward & Co. have not published Vere Foster's Drawing Books (see reply to query 274) for 3½ years; these books are published by Messrs. Blackie & Son.

DID THE EGYPTIANS KNOW OF THE MOVEMENT OF THE EARTH IN SPACE?

IN confirmation of the translation of the Berlin papyrus by M. Chabas, a paper is published in the "Transactions of the Congress of Orientalists at St. Etienne," by M. Lieblein, one of the greatest authorities on the Egyptian vocabulary, pointing out another text alluding to the motion of the earth in very similar terms. This second sentence occurs in a kind of chant to the god Ptah, found in what is known of the great Harris papyrus, contained in the British Museum, and speaks of him as "creator of the gods, maker of heaven, and founder of the earth circulating in the great ocean of heaven." This text had formerly been translated by Dr. Birch and Professor Eisenlohr as "encircling the earth with the waters of the great sea," and thus it will be seen at once that the difference between the two readings is that where one translator reads travelling or *circulating in*, the other reads *surrounding with*. M. Lieblein, consequently, in order to prove his case, gives a large number of instances of exactly similar words, where if the verb were really to surround, and the preposition with, instead of in, such a translation would make nonsense, and also refers to many texts where this very word has been rendered to circulate, several of these having reference to the path of the sun. In controverting any decision arrived at by one so pre-eminent as Dr. Birch, the very greatest caution must be observed, but no one, after reading M. Lieblein's essay, can doubt that there is at least as much right to his idea of the true interpretation of the sentence as to that of our greatest Egyptologist.

Before approaching our subject from a point of view not purely Egyptian, it will be well to point out that these remarkable texts, in speaking of the stars or sun navigating the heavens in boats and the innumerable allusions to the sky as a celestial ocean, must not be taken to do more than clothe in elegant imagery the phenomena of the universe. Their writers no more considered the interstellar space as a liquid than we do. The idea itself undoubtedly originates in the beautiful account of the creation given in the seventeenth chapter of the Ritual of the Dead, when it states that the Deity, there alluded to as Toum, separated the terrestrial waters which gave rise to all rivers and oceans, from the celestial waters, suspending the latter in heaven. That behind and beyond all these allegorical references to the surroundings of our planet existed a conception well-nigh parallel to our own, can be seen by comparing many texts; for instance, in the inscription of Amenemheb, known as "The Praise of Thotmes III.," in speaking of the King's death at evening he says:—"Then he fled up to heaven when the disk of the sun went down, and the servant of God joined himself to his Creator" (see Eccles., xii., 7). If it were legitimate to consider that the Egyptians spoke of a liquid heaven literally, then they contradict themselves, for almost as frequently it is spoken of as forming the body of the goddess Nout, whose figure is delineated adorned with the stars, her hands touching the horizon on one side, her body extended above, and her feet forming the horizon on the other. Then, again, there is another figurative theory mentioned by M. Maspero as being common, which speaks of the stars being fixed as lamps suspended in the celestial vault, and lighted each night by divine power to illuminate the earth.

A Member of the Society of Biblical Archæology.

The Solar Spectrum.—For more than twenty years I have had on my study table a prism, brass-mounted, and moving on a ball-and-socket joint. The apparatus may be had at any optician's. For two months in the spring, and for an equal period in the autumn, the sun is in a suitable position for showing through the prism a fine spectrum on the wall. It is one of the most lovely sights in nature. It never tires or becomes indifferent; and if I felt disposed to idolise a phenomenon, it would not be a sun-flower, but a sun-spectrum. Its interest to myself has been heightened by a spice of mystery. In the inexpressibly tender shading-off of the violet rays may be seen a tremulous motion; not always, but whenever the spectrum is very fine. Some of my family and friends can see it, but not all. This secret of many years I now suspect to be the passage across the violet of undulations of air ascending from the lower and more heated red portion of the spectrum. Any convenient method by which a curl of white smoke can be disengaged, so as to ascend gently through the path of the refracted rays, will afford a floating spectrum of exquisite beauty. Seen through the prism, the bars of the window, especially about sunset, are gorgeous. —Henry H. Higgins.

The Lamson Case.—Dating from the Lamson trial, we may expect to find the history of cadaveric alkaloids will be more fully investigated; and, although in this particular case none of the conditions essential to their existence were fulfilled, it will not be without some advantage, by stimulating inquiry into a very important and at the same time all but unknown class of decomposition products. In the whole masterly structure of the defence there was no more ingenious point raised than this of cadaveric alkaloids. Against the possibility of any reliance on it, however, were three fatal objections, which effectually and for all disposed of it. These were that the body of the murdered boy was not decomposed when the poison it contained produced fatal effects on animals it was injected into; that the vomited matter, preserved in alcohol, and thus free from decomposed substances, gave indisputable proof of aconitine; and that we have no proof that cadaveric alkaloids are capable of producing the physiological effects of normal vegetable alkaloids. Application has been made to the Home Secretary for a respite of the sentence passed on Lamson, on several grounds, among the reasons given being two chiefly interesting to us—viz., that the jury were not composed of medical experts, and the unreliability of the experiments made with mice. The first of these can need no serious consideration. The evidence was of a kind, more than is usually so in murder cases, to approve itself to the non-scientific mind. The symptoms of poisoning by aconite were carefully explained to the jury, and the evidence was such as to prove conclusively that these very symptoms were observed. Dr. Stevenson's testimony, moreover, was especially such as a child might comprehend; the only difference an expert jury would have made would have been to shorten the time in which the verdict was arrived at, and which even now is made one of the grounds of application for remission of sentence. Of the objection to the experiments made by Dr. Stevenson it is needless to speak further. No competent judge of their value and importance can hesitate a moment as to their weight; we venture to think none does so. On every ground, the Lamson trial will take rank as of high importance in a medico-legal sense; and as to the justness of its result we think no question can be raised.—*The Medical Press and Circular.*

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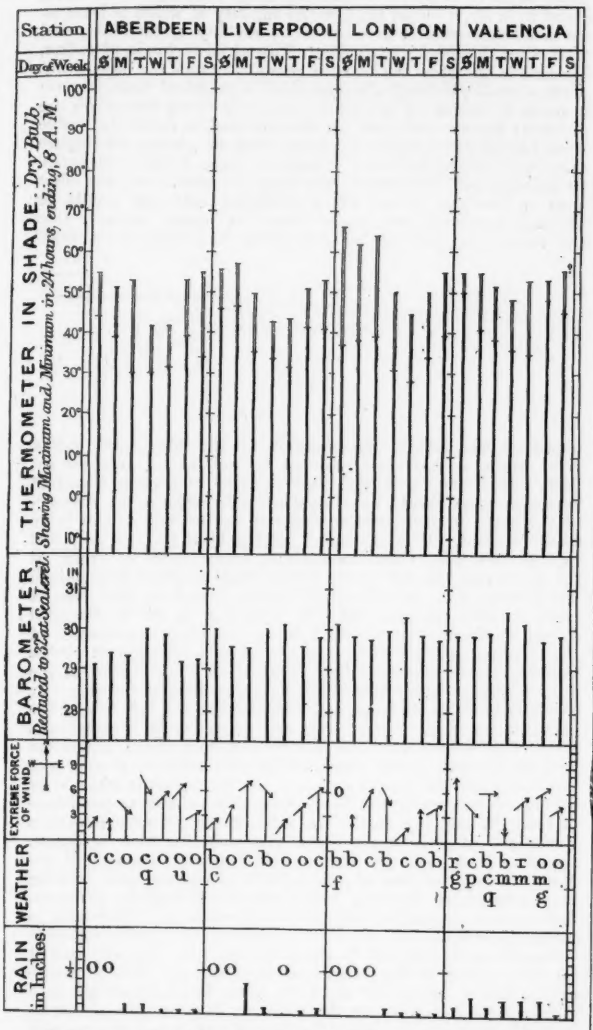
* "The Two Hemispheres: A Popular Account of the Countries and Peoples of the World." By G. G. Chisholm, M.A. (Blackie & Son, London.)

WEATHER DIAGRAM,

FOR WEEK ENDING SATURDAY, MARCH 25.



WEATHER.—*Beaufort Scale* is, b. blue sky; c. detached clouds; d. drizzling rain; f. fog; g. dark, gloomy; h. hail; l. lightning; m. misty (hazy); o. overcast; p. passing showers; q. squally; r. rain; s. snow; t. thunder; u. ugly, threatening; v. visibility, unusual transparency; w. dew.

MESMERISM.

REPLYING to "A Startled One," KNOWLEDGE, p. 301 (260) the only work I have seen treating this subject scientifically is that ("Animal Magnetism") by Professor Heidenhain (Kegan Paul & Co.), with a preface by G. J. Romanes, F.R.S.

The subject is deeply interesting, and well worthy of attention by every medical man.

1. Heidenhain arrives at the conclusion that the cause of the phenomena of hypnotism lies in the inhibition of the activity of the ganglion-cells of the cerebral cortex, the inhibition being brought about by gentle prolonged stimulation of the sensory nerves of the face (by passes), or the optic nerve (by looking at a bright object), or the auditory nerve (by a monotonous sound), and that in consequence of the depression of activity of the brain there is a great increase of reflex irritability which may be made to extend over the body upon cutaneous stimulation of local parts, causing all the usual mesmeric phenomena, such as muscular rigour, insensibility to pain, hallucinations, &c.

2. The effect on the subject is temporary, causing only an increase of nervous irritability which passes off in a few days. The effect is similar to that produced by inhaling nitrous oxide gas.

3. The subject is restored to his senses by a change of stimulation, such as stroking the skin in a direction contrary to the original one; by sudden change of temperature, as by blowing on the face; or left alone, he will in a short time come out of the hypnosis by himself.

The opinions here expressed are entirely opposed to those of professional mesmerists, who maintain that the operator is possessed of some sort of electrical influence by which he can act voluntarily on the person experimented on. Actual facts adduced both for and against this theory would be highly interesting, for Heidenhain's hypothesis does not afford a satisfactory explanation of many of the extraordinary phenomena of hypnotism.

Perhaps you will allow discussion on this point in your interesting paper?

F. GIBSON.

THE CARNIVOROUS PARROT.

IN November, 1879, Mr. J. Wood showed at a meeting of the Pathological Society the colon of a sheep in which the operation of colotomy had been performed by the *Nestor Notabilis*. The specimen, along with one of the birds, was from Otago, New Zealand.

The *modus operandi* appeared to be as follows:—The bird, settling on the sacrum, tears off the wool with its beak and eats into the flesh until the sheep falls from loss of blood and exhaustion. It is doubtful whether the birds attack dead sheep, and it was stated that they single out the strongest sheep in the flock rather than those that are sick, dying, or disabled.

It is difficult to account for this acquired carnivorous habit. Professor Flower has suggested that the bird has in view the object of getting at the contents of the intestines, while others state that the loin and the underlying intestines are especially wounded owing to the position the bird takes upon the sheep's back. This is not an explanation, however, of the cause of the habit, for although we may, and do, easily alter the dietary of individual parrots, it would be interesting to know why the *Nestor Notabilis* in its freedom on the sheep-runs of New Zealand should voluntarily change its mode of living. Perhaps some of your naturalist subscribers will give us the reason.

ARRECTIS AURIBUS.

EASY LESSONS IN BLOWPIPE CHEMISTRY.

BY LIEUT.-COLONEL W. A. ROSS, LATE R.A.

LESSON V.—THE PHYSICS* AND CHEMISTRY OF THE THING.

ALTHOUGH the merely mechanical study of blowpipe manipulation; its, comparatively with other chemical studies, child-like simplicity and absolute economy, combined with the surprising rapidity, beauty, and correctness of results, and portability of apparatus required, have all such a charm for the man who seriously takes it up, as to exercise in most cases a complete fascination over him—the physics and chemistry of the thing must on no account be neglected; and although, technically speaking, this is called the "dry" method of chemistry, it will, when properly appreciated, be found infinitely more amusing, and quite as instructive, as the "wet" method, which involves the use of violent acids, expensive and delicate glass-apparatus, a knowledge of "atomic theories," &c.

Let us now, therefore, collect and recapitulate the physical and chemical phenomena casually mentioned in the four preceding lessons, as follow:—

1. A cone of blue fire produced from a candle, oil-lamp, or coal-gas flame by a constant confined blast.

2. The probable cause of the change of colour in hot beads cooling.

3. The power of the pyrocone to produce and abolish colour in solutions of oxides.

4. The solubility of *silica* in P. salt, and its insolubility in P. acid B.B.

(1.) As regards this fact, there is an obvious and very general error, arising from optical misapprehension, or, as Mr. Foster would call it, "untrustworthy information of the senses"—entertained as to the shape and nature of the fire-cone alluded to, in consequence of all or most writers on the subject making pictures of the blowpipe "flame" (as they call it) taken at the *angle of depression*, in which they view the pyrocone, while using the mouth-blowpipe. This "optical illusion" has the effect of causing the blast to appear *within* the pyrocone, whereas, in reality, it passes over the surface. I have

* Greek, *Phusikè*, Nature; Natural Philosophy.

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now before me ten standard works on chemistry and blowpipe analysis, in not one of which is the figure of the pyrocone correctly drawn; but, as it would never do to take up the space of KNOWLEDGE with erroneous drawings, I shall here merely insert two; (*a*) from

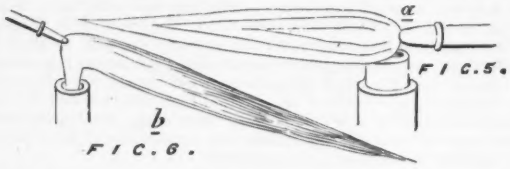

FIG. 5.

FIG. 6.

Bloxam's Chemistry; (*b*) from Thorpe's. In these pictures it will be seen that the "nozzle" of the blowpipe appears to be sending a blast into the *centre* of the pyrocone, which latter is therefore said by most chemical writers to be as hollow as the luminous flame of a candle undoubtedly is; a statement which, as shall be abundantly proved in the course of these "lessons," is also quite incorrect.

Now, let us light a common candle in daylight, and apply a blowpipe blast to it, near a window, viewing the resulting pyrocone by transmitted light (that is, holding the candle between us and the window) *on a level with the eyes*; or, speaking concisely, in the

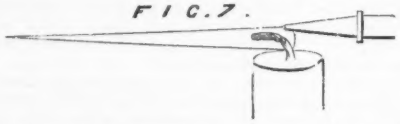
FIG. 7.

horizontal plane of vision. This is something like what we see, but by slanting the candle a little, we can see quite distinctly the actual path of the blast on the *upper surface* of the pyrocone; and moreover, we can easily prove, while holding the candle or lamp upon this level, that it is *impossible* to force the blast inside of the pyrocone. Another proof which I take from a former paper (Proc. Roy. Soc., Vol. XX.) is that, in looking directly down (or perpendicularly) on a pyrocone formed upon a thick wick, as that of a

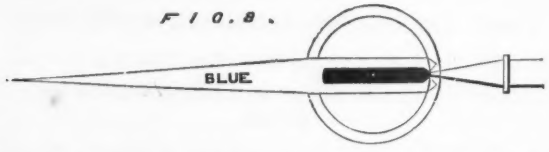

FIG. 8.

Berzelius' lamp, we see the black carbonised wick through the space which is always formed in the centre of every luminous flame, and not through blue fiery matter, as would be the case were the blast in the *centre* of the blue pyrocone. Yet another proof lies in the fact that upwards heat-radiation is almost entirely stopped by the superposed blast.

Simple learners like the poor workmen to whom I chiefly address these little "lessons"—though I myself have quite as much to learn in the matter as to teach—will scarcely credit the fact that none of the eminent authors who have condescended to include blowpipe-operations in their chemical writings, have thought it worth their while to make the least inquiry as to the *cause* of the phenomenon (1) now under discussion. No doubt, if such men as Plattner or Berzelius had done so, speaking as they would, too, *ex officio* (which you must not translate by "as a Jack in office"), they would have at once cleared up the matter; as it is, the following suggestion of an ex-artillery officer must be taken in lieu of anything better. As before, we must rest our inquiry on what that corrupt, old, marvellous judge (I wish we had such judges now-a-days!) and genius, Lord Bacon, called "Induction;" that is, reasoning from experiment. Most young Englishmen possess uncommonly good eyesight, so that they will find little difficulty in proving the truth of the following extract from my smaller book, "Alphabetical Manual of Blowpipe Analysis," page 102. "If the blast from a mouth-blowpipe be propelled by an operator with good eyesight across the heat-undulations rising from a lighted candle in broad daylight by *transmitted* light, he will *see* it (the blast) in the shape of a *straight line*, about the thickness of a fine sewing-needle. If now he propel a similar blast through the blue thin flame of a spirit-lamp, which is penetrable by the blast, and observe it by *reflected* light, that is, when he is between it and the window, he will again see it, but this time in the form of a minute *cone* of air, synaxial (that is, formed upon the same line, as it were) with a flame-cone projected in the same direction, from the spirit-lamp. What causes this difference in the appearance of the same thing? The only reply to this question seems to me to be that a confined, continued blast appears to create round it, in air, a *vortex*, the gyrative rapidity of which is least at the commencement, or greatest *direct* rapidity of the blast, and most when that begins to slacken. This aërial vortex is, of course, invisible in air when the blast is passed through the heat-waves above a candle, although the blast itself is perceived there, because a space is formed by it within which the waves themselves are checked; but the air-vortex, on the contrary, is distinctly visible when formed within a coloured (blue) fluid of greater consistence than itself, like spirit-flame. It seems obvious, if this explanation be correct, and the other fact kept in mind, that the blast from a blowpipe is not really projected *into* the candle or oil-lamp pyrocone, but passes over its upper surface, that the vortex thus created includes within its gyrations the flame

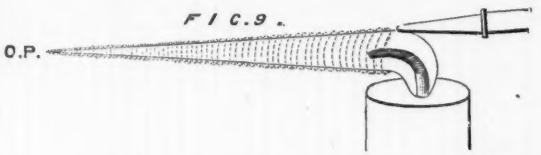

FIG. 9.

blown on one side in the direction of the blast, and forces it to retain a horizontal position, as well as its own conical form. It follows from this, that in order to produce a perfect pyrocone, there should be a constant ratio between the strength of the blast (or air pressure) and the size or bulk of the flame acted on—and this is the fact." If the blast be now propelled into a spirit-lamp flame held about half-an-inch in front of the point marked OP in the figure, that is, about half-an-inch distant from the apex of the aërial vortex, a short *inverted* cone of large diameter appears; and this, according to Sir J. Herschel (Essay on Meteorology, page 67) is "a necessary consequence of the vorticose motion." The existence of this atmospheric vortex may be further confirmed by holding about three inches of the *finest* platinum wire, which has very considerable "spring" or elasticity in it, so that one end just touches the *left side* of the blowpipe pyrocone. That end will immediately commence a series of gyrations, from left to right, rapid in direct proportion with the strength of the blast, and *therefore* with the shortness of the diameter of the base of the cone. Another proof is obtained by holding an extremely hot bead—as nearly red-hot as possible—of fresh P. acid just under the base of the pyrocone; when a "mantle" of pale green flame surrounds the blue pyrocone, spreading from base to apex. This "mantle" consists of infinitely small particles of the volatile phosphoric acid burning in the vortex of intensely heated air surrounding the pyrocone. But the most conclusive evidence of all—evidence which seems to me simply confirmatory of the *fact*—is this:—An elastic, expansible cone, such as that formed by an air-vortex, *must*, if compressed at its base, *extend in the direction of its gyrations*. This is a self-evident law of Dynamics,* and I can never forget the delight with which I found, when I placed a globular platinum dish so that its curved bottom almost touched the base of the blue pyrocone, and thus was bound to *squeeze* the air-vortex at its commencement, that my pyrocone instantaneously *increased in length at least half an inch*.

We thus possess in this hypothesis†—for, of course, chemists will not allow, for another ten years at least, that there is here the absolute proof of the existence of these facts which they have obtained of the existence of proportional atoms—a simple and "pretty" explanation, not merely of the conical shape of the "flame" or fire, but of the oxydising properties of the position OP; of the still more intensely oxydising properties of the position PP, where the inverted aërial vortex exists; and of the hydrogenising or "reducing" properties of the position HP; that is, of the inside of the *solid* blue pyrocone, which, according to the present accepted "theory" of blowpipe pyrocones, must be full of oxygen contained in the air of the blast.

But, as the poor ghost in "Hamlet" says (at the beginning, by the way, instead of the end of his oration), "My hour is almost come;" and space permits no more on this head at present, so that I must reserve the discussion of the other chemical and physical effects mentioned, for Lesson VI.

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* Greek, *Dunamis*, power; he branch of Physics which treats of bodies in motion, as opposed to *Statics*.

† "Hypothesis, a supposition. Something not proved, but assumed for the purpose of argument" (Ogilvie's Dictionary).

XUM

STARS FOR APRIL

Our Star Map.—The circular boundary of the map represents the horizon. The map shows also the position of the equator and of that portion of the Zodiac now most favourably situated for observation. For the motions of the planets Jupiter, Mars, and Uranus, consult the Zodiacal maps in Nos. 11 and 19. The names of ninety-nine stars of the first three magnitudes are given below.

On March	31,	at 10.30 p.m.
On April	3,	at 10.15 p.m.
On April	7,	at 10.0 p.m.
On April	10,	at 9.45 p.m.
On April	14,	at 9.30 p.m.
On April	18,	at 9.15 p.m.
On April	22,	at 9.0 p.m.
On April	26,	at 8.45 p.m.
On April	30,	at 8.30 p.m.
On May	3,	at 8.15 p.m.
On May	7,	at 8.0 p.m.

ARABIC NAMES OF STARS.

The following table exhibits the names of all the stars of the first three magnitudes whose names are in common use:—

α Andromedæ	*Alpheratz*
β ———	*Mirach, Mizar*
γ ———	*Almach*
α Aquarii	*Sadalmelik*
β ———	*Sadalsund*
δ ———	*Skat*
α Aquilæ	*Altair*
β ———	*Alshain*
γ ———	*Tarazed*
α Arietis	*Hamal*
β ———	*Sheratan*
γ ———	*Mesartim*
α Aurigæ	*Capella*
β ———	*Menkalinan*
α Boötis	*Arcturus*
β ———	*Nekkar*
ε ———	*Izar, Mizar, Mirach*
η ———	*Muphrid*
α Canum Venat.	*Cor Caroli*
α Canis Majoris	*Sirius*
β ———	*Mirzam*
ε ———	*Adara*
α Canis Minoris	*Procyon*
β ———	*Gomeisa*
α^2 Capricorni	*Secunda Giedi*
δ ———	*Deneb Algiedi*
α Cassiopeiæ	*Schedar*
β ———	*Chaph*

α	Cephei	*Alderamin*
β	———	*Alphirk*
γ	———	*Errai*
α	Ceti	*Menkar*
β	———	*Diphda*
ζ	———	*Baten Kaitos*
ο	———	*Mira*
α	Columbæ	*Phact*
α	Coronæ Borealis	*Alphecca*
α	Corvi	*Alchiba*
δ	———	*Algores*
α	Crateris	*Alkes*
α	Cygni	*Arided, Deneb Adige*
β	———	*Albireo*
α	Draconis	*Thuban*
β	———	*Alwaid*
γ	———	*Etanin*
β	Eridani	*Cursa*
γ	———	*Zaurac*
α	Geminorum	*Castor*
β	———	*Pollux*
ζ	———	*Alhena*
δ	———	*Wasat*
ε	———	*Mebsuta*
α	Herculis	*Ras Algethi*
β	———	*Kornefores*
α	Hydræ	*Alphard, Cor Hydræ*
α	Leonis	*Regulus, Cor Leonis*
β	———	*Deneb Alest, Denebola, Deneb*
γ	———	*Algeiba*
δ	———	*Zosma*
α	Leporis	*Arneb*
α	Libræ	*Zuben el Genubi*
β	———	*Zuben el Chamali*
γ	———	*Zuben Hakrabi*
α	Lyræ	*Vega*
β	———	*Sheliak*
γ	———	*Sulaphat*
α	Ophiuchi	*Ras Alhague*
β	———	*Cebalrai*
α	Orionis	*Betelgeux*
β	———	*Rigel*
γ	———	*Bellatrix*
δ	———	*Mintaka*
ε	———	*Alnilam*
α	Pegasi	*Markab*
β	———	*Scheat*
γ	———	*Algenib*
ε	———	*Enif*
ζ	———	*Homan*
α	Persei	*Mirfak*
β	———	*Algol*
α	Piscis Australis	*Fomalhaut*
ε	Sagittarii	*Kaus Australis*
α	Scorpionis	*Antares, Cor Scorpionis*
α	Serpentis	*Unukalhai*
α	Tauri	*Aldebaran*
β	———	*Nath*
η	———	*Alcyone* (Pleiad)
α	Ursæ Majoris	*Dubhe*
β	———	*Merak*
γ	———	*Phecda*
ε	———	*Alioth*
ζ	———	*Mizar*
η	———	*Alkaid, Benetnasch*
ι	———	*Talitha*
α	Ursæ Minoris	*Polaris*
β	———	*Kochab*
α	Virginis	*Spica Azimech, Spica*
β	———	*Zavijava*
ε	———	*Vindemiatrix*

XUM

XUM

COD-SOUNDS AND SCIENTIFIC PRIVILEGE.

WHEN I stated (page 295) that the tough leathery membrane of the cod-fish, known to epicures as the "sound," is an organ of different structure and anatomical relation to the swim-bladder of other fishes, I had no idea that the subject was sufficiently interesting to call forth the critical correspondence it has elicited, and should hardly have prolonged the discussion, but that another and far more important subject has been connected with it. "Old Fossil" says (page 380), that I have illustrated "the old saying that a cobbler should stick to his last," and that on "passing into the domain of the biologist" I must "be regarded as an intruder." Dr. Wilson (page 429) quotes and supports this statement which assumes that scientific inquiry, scientific discussion, or scientific criticism is the exclusive privilege of labelled specialists, who must never invade each others' domains.

This is a mischievous dogma, too often asserted with less courtesy than by the gentlemen above-named, and sometimes even with downright insolence by certain narrow-minded pedants. "There is no man old enough to be an expert in all the sciences, and yet all the sciences are but one science, and all our subdivisions are merely artificial devices for the convenience of study. Hence, if every man confined himself to his own particular branch of special knowledge, the divine unity of creation would remain unknown, and the highest object of all science—the uplifting and purification of the human mind by the unselfish contemplation of the marvellous harmonies of the universe—would be unfulfilled. The new-born science of celestial chemistry could not have come into existence without the previous wedding of the laboratory to the observatory; and if we take a general survey of the progress of human knowledge during the present generation, it will be seen that the greatest strides have been made by those who have boldly stepped across the conventional boundaries that mark the customary subdivisions of the sciences." I wrote the above protest thirteen years ago; I now repeat it with especial emphasis in the columns of KNOWLEDGE, the value of which I regard as largely due to its general freedom from the pedantry of the self-sufficient specialist.

Has not Tyndall invaded the domain of the biologist in conducting his researches on atmospheric germs, and have not the truly philosophical biologists good reason to thank him for doing so? All such biologists acknowledge the importance of Herbert Spencer's profound contributions to the theory of evolution; but can he be labelled a biologist? I need only mention the names, of Humboldt, Bunsen, Kirchoff, Helmholtz, Huggins, Huxley, &c., as illustrations of men who, by forsaking their special lasts, have ceased to be scientific cobblers, and have thereby become true philosophers. This very magazine could have had no existence had its editor submitted to be strapped down to the astronomical last with which his earlier literary efforts are associated, and some of his best essays must have been suppressed had he not invaded other "domains."

Such specialists are unquestionably necessary to the building up of the glorious edifice of inductive science, just as special masons, bricklayers, carpenters, joiners, &c., are demanded for physical buildings, and I should be the last to dispute their dignity and importance, even when protesting against their undue assumptions of exclusive privilege.

Dr. Wilson and "Old Fossil" are quite right in asserting that I am not a biologist, though my earliest studies were biological, and date from a period preceding the invention of the term "biology" and the birth of Dr. Wilson, viz., 1841, when I was a pupil of Professor Jamieson in "Natural History," and attended the lectures of "Monro *tertius*" on "Anatomy and Physiology" in the University of Edinburgh.

"Old Fossil" tells us that he dissected a cod-fish of 8 or 9 lb. weight on the day of writing. I have dissected many twenty-five to thirty years ago, and therefore depend upon memory. The reader, however, may judge for himself, by simply cutting such a fish in half, or asking a fishmonger to do so for him, and he will then be able to judge by the diameter of the blood-clot enclosed by the stout membrane in question whether it can possibly be contained within an aorta of $\frac{1}{12}$th of an inch diameter. If the section is made at about the posterior termination of the abdominal cavity, he will find that this blood-clot is nearly half-an-inch in diameter, and confined between the spines and the thick membrane in question. If he follows this membrane forward, he will find it still adherent throughout its whole length to the spines, and underlapping the blood-clot, which now becomes divided, and lies on each side of the body of the vertebræ, accumulated in the hollows formed by the bases of the vertebral arches. It also contains air, and this has probably led "Old Fossil" to suppose that it is a true swim-bladder, the organ which modern biologists regard as an homologue to the lung-bag of the amphibia and reptiles; one of Dr. Wilson's "found links."

All such bladders differ essentially in structure and anatomical relations from the cod-sound. They are formed of a thin, translucent, delicate membrane, corresponding to the pleura, or membranous envelope of the lung-bag of air-breathers; the cod-sound has a tough leathery coat, like that of our own arteries. It is a contradiction to all anatomical analogies to suppose that a mere air vesicle should have walls strong enough for a fire-hose. The true air-bladders, like the lung-bags of the amphibia, are, as John Marshall says, "off-shoots from the upper part of the digestive canal," and come away freely from the abdominal cavity along with the other viscera when these are removed. The cod-sound is connected with the heart (as a prolongation or modification of the *bulbus arteriosus*) of the fish, and so firmly attached by its edges that it has to be forcibly torn or "söndert," by the Norsk fishermen, after all the rest of the viscera are removed, and thus, as I believe, obtains its name. When not thus sundered for salting, it is ripped open in order to remove the blood which it contains throughout its entire length. I have examined the swim bladder of many fishes—the single-lobed bladder of our common fresh-water fishes; the slippery double bladder of eels; the curious three-lobed bladder of the gurnets (the proportionate capacity of which is at least twenty times that of a cod-sound, and yet is composed of thin membrane); and the still more delicate, collapsing bladders of the herring and other similar fishes—but have never found any blood within them, still less that they enclose the great dorsal clot which I find in all fishes after death enclosed in a special membrane corresponding to the cod-sound, though generally thinner, and always quite independent of the swim bladder.

In spite of the scalpel of "Old Fossil," I still regard the sound of the cod-fish, and the corresponding membrane of other fishes similarly adherent to the spine, as the main bloodvessel of the animal, for the simple reason that its blood is always contained therein, but I do not deny that the air which it also contains may assist the buoyancy of the fish, seeing that this buoyancy is obtained in other fishes by other arrangements than that of the ordinary lung-like swim-bladder.

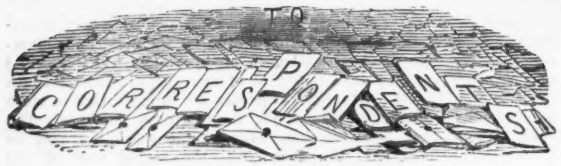
As regards the contents of this dorsal aorta during life, I may mention one experiment. I have on several occasions lashed myself to the martingale of a schooner sailing in the Mediterranean, and stood so near to the water that my feet have dipped when the vessel pitched. From this favourable position I have speared bonettas, and observed that when one of the five barbs of the "grains" (as the sailor calls the murderous implement), has pierced the adherent membrane in question, that the water all around the fish has suddenly become deeply stained with blood to a distance of a foot or eighteen inches, and the usual blood-clot under the spine has diminished accordingly, indicating more contractile work than could be done by the tiny heart attached to the gills, and suggesting the probability of direct and powerful contraction of this sub-dorsal membrane, which I believe does much more in circulating the blood through the body of fishes than Dr. Wilson and "Old Fossil" imagine. Rymer Jones (who was a biologist especially strong in comparative anatomy) tells us that there is "no systemic heart in fishes, the aorta itself serving to propel the slow-moving blood in its course through the arterial system." Hence the demand for the strength and thickness of the cod-sound, which I commend to the biological attention of the readers of KNOWLEDGE when they next partake of boiled cod-fish. Even though not "accustomed to the use of the scapel," they will then be able to consider the probability of this being merely devoted to holding air, while the "delicate thin-walled tube, about one-twelfth of an inch diameter," which "Old Fossil" found in the 8 or 9 lb. fish, propels the blood throughout its body without any help from the outer tough membrane of the sound.

W. MATTIEU WILLIAMS.

ERRATUM.—In letter on the Radiometer, p. 457, last line but one, for Sodium read Iodine.

COLLIERY "SPOIL BANKS."—You are doubtless familiar with the large heaps of refuse removed from coal-pits, technically known as "spoil banks." These heaps are usually, though not always, on fire, and when seen after dark present an appearance of wild and magnificent grandeur. I have often heard expressions of wonder and admiration from persons who have seen these burning mountains for the first time. It is my habit to look at these "mountains" from a less romantic standpoint, as the sulphurous and other noxious fumes arising from them are doubtless as injurious to animal life as the appearance of the immediately surrounding district proves them to be to vegetable life. I frequently hear the assertion that these heaps fire "spontaneously." Will you kindly give your opinion, through the columns of KNOWLEDGE, as to whether this is likely to be; and, if so, how it occurs? How is the chemical combination necessary to produce flame brought about?—ONE WHO WANTS KNOWLEDGE.

XUM



Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE*; all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

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**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE*, should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Queries and replies should be even more concise than letters; and drawn up in the form in which they are here presented, with brackets for number in case of queries, and the proper query number (bracketed) in case of replies.

(III.) Letters, queries, and replies which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

SCREW-DRIVER.

[359]—I am almost ashamed to make my first communication to KNOWLEDGE on such a humble subject as a screw-driver. But though a humble servant, it is one that we are never likely to be able to do without; and I should think there are few of its employers who have not been inclined sometimes to use strong language at its perversity in slipping out of the nick, and in refusing to enter it. I wonder that it never occurred to me until the other day, when I saw a man continually meeting with this trouble, that it may easily be prevented by the simple contrivance of putting on a tube to embrace the screw-head.

There are, however, a few details to be attended to. Screw-heads, for which the same driver is suitable, are not all of the same size. Moreover, the point of the screw-driver of the now usual round shape (which is the nicest to handle) is widened out, so that a tube which will go over it will be loose on the shank above. The way to meet both those difficulties is to put a piece of leather round the shank while you slip the tube on, of such thickness that it will hold moderately tight. The consequence of that also will be that as the screw-head goes down into the wood, the tube will be spontaneously pushed up. For larger screws than the driver is generally used for, you only want a larger tube and thicker leather.

I never believe in anything that involves motion till I have tried it. I have tried this with a tube made only of strong tin, like an old-fashioned slate-pencil case, and it answers perfectly. You need not even look at your screw. Just put the socketted driver on and turn; it straightway walks into the nick, and stays there until the screw-head is imbedded in the wood, or metal, as the case may be.

I also abhor patents, and I therefore neither patent this, nor tell it to a tool-maker. The last time I suggested a practical improvement (not to a tool-maker, certainly) the man straightway went and patented it. Luckily, however, it turned out not to be new, and so the older makers laughed at him. EDM BECKETT.

COLOUR OF PALÆOLITHIC MAN: CHRISTMAS ROSES.

[360]—There is no evidence that Palæolithic man was black; it is a sheer stretch of imagination on the part of the present writer. But when we consider that black is the common colour of the skin in all the anthropoids, that all the existing lowest human races are black, and that the early Palæolithic skulls of the so-called Canstadt type closely resemble those of the modern Australians, I think we are fairly justified in assuming that the drift-men at least were black and woolly-haired. Whiteness is, after all, a mere acquired trait of the very highest races, and it may well be doubted whether anybody was white till a very recent period, a mere trifle of forty thousand years or so ago. Prof. Boyd Dawkins, indeed, believes that the cave-men or later Palæoliths resembled the Eskimo; but if he means us therefore to conclude they were as light-skinned as those modern people, I think the burden of proof certainly lies with him.

The little cup-like organs in the Christmas rose are really degraded petals; the white petal-like outer leaves are really sepals. The petals secrete honey, and therefore were described by Linnæus as nectaries. It is a common habit of the buttercup family (to which the Christmas rose belongs) to develope coloured sepals for the attraction of insects, and then either lose the petals altogether, or dwarf them into small honey-secreting organs. Green hellebore and bears-foot, two closely allied plants, grow wild sparingly in England, and have the same arrangement of parts as the Christmas rose. The bee alights on the centre of the flower, visits the eight or ten tubular petals one after another, and dusts himself in doing so with pollen from the stamens, which he then carries to a neighbouring blossom. The pistils of each flower mature before the stamens, and so, even if the bee drops some of the pollen on the same flower, it does not interfere with cross-fertilization, because the pistil will in all likelihood have been already impregnated. But when he passes from an old blossom, in which the stamens are shedding their pollen, to a young one, in which the pistils are mature while the stamens are yet unripe, he at once fertilises the ovary, and thus ensures a plentiful crop of healthy seed. GRANT ALLEN.

HIGH NUMBERS.

[361]—A parenthetical remark of yours in an early number of KNOWLEDGE prompts me to ask you if you will be kind enough to say what you consider to be the scientific method of notation in respect to very large numbers. That is to say, since a million is a thousand thousands, should we call a thousand millions a billion, and a thousand billions a trillion, and so on; or should we reckon a billion as a million millions, a trillion as a million billions, and so on, multiplying by a million for each one? Or, peradventure, is neither of these methods right? WINTER.

[It has always seemed to me the English system of calling a million million—that is, a million to the *second* power—a billion, a million to the *third* power, a trillion, and so on, is sounder than the American system of calling a thousand millions a billion, a million millions a trillion, and so on. No meaning can be given to the *bis, tris,* &c., in the compound word on the American plan, whereas there is a very obvious and natural meaning on the English plan.—ED.]

JUPITER IN CASSIOPEIA.

[362]—I presume that the Rev. H. H. Higgins, who sets down the statement that "Wallenstein saw Jupiter in Cassiopeia," as an absurd mistake, (*vide* "Answers to Correspondents," p. 392), refers to a passage in Schiller's "Wallenstein's Tod," where the famous warrior is made to say (Act V. sc. 3):—

"Kein Sternbild ist zu sehn! Der matte Schein dort,
Der einzelne ist aus der Kassiopeia,
Und dahin steht der Jupiter."

If such be the case, I hope you will allow me to state that the error is not Schiller's but must be laid to the charge of his commentators and translators, who have construed the word *dahin* as if it meant "therein," whilst it is employed in the sense of "yonder;" for the speaker points with this expression to another direction than that where he had seen Cassiopeia. I may add that I have interpreted the above passage in the same sense in my commentary to Schiller's "Wallenstein."

C. A. BUCHHEIM, Ph.S.

[Does Dr. Buchheim mean that the interpretation he puts upon Schiller's words is the one which would naturally be put upon them by German readers?—ED.]

SPACE PARADOX.

[363]—I fancy the following problem might interest your readers.—Two bodies, A and B, have been travelling in the same path through space from eternity. B goes a mile an hour faster than A. Since they have been travelling for an infinite number of hours, there is an infinite number of miles, or any other unit of length between them. Hence a line drawn from A to B will be infinite, although bounded at each end by A and B.

XUM

Would you be kind enough to give an explanation of this apparent paradox? If this is not one, is it ever true that the conditions of human thought involve contradictions where it transcends the finite? Of course, Kant says that our ideas of space and time are forms of thought founded on no realities. But apart from this hypothesis, can a logical explanation of such problems as the above be supplied? Perhaps this might be combined with the suggestion of J. S. T., in letter 295, if, as I and many others hope, you are going to give an article on space. ETON.

[This paradox resembles Aristotle's proof of the finiteness of space, which never satisfied any one (as Sir J. Herschel remarks), though unanswerable,—viz., Since whenever we take any two points in the universe, the straight line joining them is finite, the universe itself is finite. But as this depends on the first postulate of Euclid, so the answer depends on the second;—Each of Aristotle's finite straight lines can be produced to any distance in the same straight line; therefore the universe is not finite. Lastly comes the third postulate, which since it asks us to admit that a circle can be drawn having any centre and at any distance from that centre, corresponds with Pascal's saying, that the centre of the universe is everywhere, its circumference nowhere.—ED.]

AN IDIOT DOG.—AN EXCEPTION PROVING A RULE.

[364]—I once possessed a black curly-haired Newfoundland and retriever, weighing 125 lb., and standing 2 ft. 4 in. high, as you would measure a horse. Despite his beauty and magnitude, he was, however, if not an idiot, certainly the least intelligent dog I ever saw. As a yard dog he was quite worthless, except that his amazing size terrified alike the honest and dishonest—*i.e.*, in daylight, for let come what or who would, he opened not his mouth. In the house he was equally crass. In his movements he seemed to have but one idea, and if a table or a child were in his way, down they would go—Lion cared not. If taken out of doors he would go straight ahead, neither knowing his master nor his way home, his sole and great delight being to slay any dog approaching him in size.

I have recently read a translation of a work on "Mind in Animals," by Ludwig Buchner. Although he only describes ants, bees, wasps, and spiders, yet he succeeds in annihilating the old "instinct" superstition. Students of the subject of mind in animals should read this book. JOSEPH WOOD.

WOOD-GAS.

[365]—I repeat that carbonic anhydride can be perceived by the organ of taste and by the organ of smell. If "F. C. S." considers that the taste and odour are due to impurity in the CO_2, as is the case with hydrogen, will he state what this impurity is? The flame of CO was observable as I wrote, over a red-hot fire without flame, which I call a "sluggish fire." The process of CO from CO_2 and heated carbon is nothing new; it is to be found in many text-books. The main point, after all, in the use of such gas, would be the danger of an escape. LEWIS ARUNDEL.

NOTES ON ROWING.

[366]—"Notes on Rowing" are very acceptable, and, happily, promise more. Rowing, too, is not the only instance of propulsion by pressure upon a lever of the second order. The tractive force of a locomotive engine can be explained in the same way. To draw a parallel, one might say that the rail corresponds to the water, for it is the fulcrum. The axle-boxes are the rowlocks; the crank-pin is the "spool" of the oar, and the periphery of the driving-wheel is the "blade." Each end of the cylinder (which, by the way, sits like a rower, forward of its work) is in turn a "footboard" against which the steam reacts with a force approximately equal to the pull or thrust upon the crank-pin. And, to make the parallel complete, I may say that the "slip" of a driving-wheel when an engine is running at high speed is at the present time the subject of experiments. Of course, the action of a driving-wheel when the crank is below the axle has no parallel in rowing, but it is a good exercise for the student of mechanics to prove that the forward pressure upon the engine is the same on each stroke, in spite of the difference in leverage. Lastly, the motion of the piston of the engine corresponds pretty accurately with the motion of a rower's body—backward and forward with respect to the boat, but continuously forward (although with a varying velocity) with respect to the water. Take any point in the periphery of the driving-wheel, too, and see how it goes from a condition of rest when in contact with the rail to acquire a velocity equal to double the velocity of the train when at its highest point.

In the same way, the oar blade, from being nearly stationary in the water, suddenly leaps forward with a velocity (that is, if nothing happens) at least double the boat's speed. Is the parallel complete?—Yours, &c., A. N. S.

ELECTRIC TELEGRAPH.

[367]—Ronald's electric telegraph (see KNOWLEDGE, XIX., p. 401), is by no means the oldest known piece of telegraph aparatus. Professor Sömmering (I am not quite sure of the spelling of the name), of Munich, constructed, in 1809, an *electric telegraph*, which is preserved in one of Munich's numerous museums, and was originally laid down between Professor S.'s laboratory and that of one of his learned colleagues. A description and drawing of it are given in "Das Buch der Erfindungen," publ. Otto, Spamer, Leipzig. I give an extract in the following:—

There were as many circuits as the alphabet has letters, further figures, signs, &c., between the two stations. Each of the circuits was an apparatus for decomposition of acidulated water, by means of the galvanic current. The tubes containing the water represented letters, &c. The rising of gas bubbles in a certain tube indicated to the receiver a certain letter. There was only one battery, which could be connected to all circuits by means of switches. There was also a cleverly-designed signalling apparatus to attract the receiver's attention. One tube contained a glass bell, under which the poles were situated, gas evolution would drive out the water of the over-turned bell, and cause it to rise, move a lever, and set a clockwork going. This is written after a lapse of years since I read the description, and there may be some slight incorrectness in the details, but I have given the general idea correctly.—Yours, &c., F. STERN, D.PH.

VEGETARIANISM.

[368]—I wish to make a few remarks on the letter 207, p. 251, in which the writer politely calls vegetarians "amiable fanatics," and further adds that they seem determined to force their way into the columns of KNOWLEDGE. This is a very grave charge indeed. Will the writer kindly state the number and date of Carper's Quarterly Journal, in which Dr. Wald's statement about the prisoners in a castle at Waltenburg is to be found; for in the interests of truth and humanity this case requires careful investigation. For example, it would be important to know whether the bread given to the prisoners was white, and if much salt was taken with the leguminous food mentioned.

It might be also useful to know the proportions of meat to vegetable diet partaken of, as a rule, by the writer of letter 207.

So far as my knowledge extends, I find that the greatest intellects of the present and the past have been either altogether or almost akreophagous.

As I write for the sake of information and not of controversy, I am thankful that the columns of such an excellent paper as KNOWLEDGE are open to the discussion of this important question—viz., What is the best food for the million?

Why should abstainers from flesh, fish, and fowl be honoured with so many conflicting titles? In the few numbers of KNOWLEDGE, in which the subject of vegetarianism has been mooted, I find them described as "phytophagists," devourers of "potatoes and turnip-tops," "eaters of greens," &c. C. L. PORCHER.

TELEPHONE.

[369]—I have been much interested in "G. E. V.'s" description of the telephone, and should like very much if he would follow it up by an account of the microphone transmittor. In making the telephone, "G. E. V." does not say how much wire he puts on his bobbins, nor the number of it. Would he oblige by stating what length and number he considers most suitable? W. B.

NOTES ON SCIENCE.—The former pupils of University College School have raised a fund for the encouragement of science amongst the boys at their old school. Besides an exhibition for practical chemistry and a prize for experimental physics, they have founded a medal, which will be awarded, at most annually, for original work of sufficient merit in any branch of experimental science done within a stated period of leaving the school. The medal, for the design of which the contributors are indebted to Mr. Thomas Woolner, R.A., will be exhibited in the Royal Academy this year, and a copy will be deposited subsequently in the British Museum. It is not for us to criticise the work of that distinguished sculptor; those who have seen the copy belonging to Mr. Temple Orme, pronounce it to be one of the finest medals ever struck.

XUM

Queries.

[342]—Gold.—Will any reader kindly inform me how I could separate gold from paper? It is daubed from the brush which has been used for illuminating. I enclose specimen.—Puzzled.

[343]—Lobster.—Why does the lobster-shell turn from blue-black to red when it is boiled?—A. D. G.

[344]—Electroscope.—Is there any explanation of the inability of a point to draw the electricity from a charged electroscope?—Boz.

[345]—Crystallisation.—Will someone in Knowledge tell us something about dendritus moss agates or frost on our window-panes? How it is they so much resemble the growth of plants?—R. W. R.

[346]—Sinking Funds.—Are there any tables published showing what sum per annum must be paid in order to repay a loan with interest, so as to clear off both principal and interest in a certain number of years by equal instalments, and what is the formula for arriving at the result?—A. N.

[347]—Cards.—We have had an argument on the question as to whether it is proper grammar to say "Club" *is* trump or "Clubs are trump," or "Clubs *are* trumps. Would the Editor, or any reader, kindly decide the question?—R. U. S.

[348]—Non-Conductor.—Would some kind friend assist me in this matter? I want to know some ingredients to make a kind of paint that will be a non-conductor of heat.—J. H.

Replies to Queries.

[218]—Sea-blue Bird of March, "*underneath the barren bush*," is explained by an ornithological friend to be probably the blue-tit (as shown in a Christmas card). The lines quoted by "Oudeis" refer to the kingfisher, but why should the other lines?—W. W. F.

[273]—Strength of Material.—In reply to "F. M.," the distance, 8 feet, is measured horizontally between the rail and the pivot; 9·17 feet is the length of the *slanting* line shown between rail and top of pivot. To make Anderson's figures correct, the surface of rail on which roller runs must be sloped so as to touch this line, the roller being, of course, set at a corresponding angle. When this is done, "F. M." will see at once that the force acting on pivot is no longer vertical, but in the direction indicated by the arrow to the left of diagram, marked 82 tons. The resolution of this into vertical and horizontal components is shown correctly. "F. M.'s" figures are correct for the diagram as it stands.—C. H. Wingfield.

[276]—Photography.—1. In reply to W. E. F., he will find it much better to purchase collodion. Price according to quality, say from sixpence per ounce. 2. The thin metal plates can be purchased at any shop where photographic materials are sold; but neither collodion nor plates are of any use without a lens, camera, and other requisites; also some experience in their use. 3. The crystals referred to are probably prosulphite of soda.—A. Brothers.

[307]—"Descriptive Geometry," by Edgar and Pritchard, 3s. 6d. or 4s. 6d. (Macmillan & Co.); also Angel's "Practical Plane and Solid Geometry" will be helpful (Collins' "Elementary Series," at 1s.). Don't attempt the subject without models, which can be made of pasteboard, strings, and wire. You would not study plane geometry without diagrams.—A. H. H.

[310]—Quartz in Coal.—Veins of quartz are frequently found in shales and coal. Such veins appear to have once been chinks, or small cavities, caused, like cracks in clay, by the shrinking of the mass, which has consolidated from a more or less fluid state, or has simply contracted its dimensions in passing from a higher to a lower temperature. Siliceous, calcareous, and occasionally metallic matters frequently have found their way into such empty spaces by infiltration from the surrounding rocks. And see Lyell's "Elementary Geology," 1855, p. 627.—Lewis E. Emmet.

[311]—Heating Room.—"J. W. B." asks me whether there is any sanitary objection to heating a room with Bunsen burner, without stove-pipe or vent. He evidently supposes that "with a perfectly blue flame" the atmospheric burner has some sanitary advantage over ordinary burners. This appears to be a commonly prevalent idea, but it is without foundation, provided always that the ordinary flame with which the Bunsen is compared is burning properly, with ample supply of air. Both supply the products of combustion of bisulphide of carbon and the other impurities of coal gas, while a paraffin lamp or candle, burning a pure hydrocarbon, only produces carbonic acid and water. A bad burner may supply a smoky luminous flame, and even a Bunsen may go wrong if it lights inside the tube.—W. Mattieu Williams.

[312]—Pyrological.—Colonel Ross has given his address in Knowledge, No. 17, p, 354, for the convenience of pyrological inquiries; to save the editor and printers the trouble of inserting inquiries on this head, and to avoid replying to anonymous communications, which he declines to do.

[313]—Botanical.—Jemima will find just the things required in "Manual of Botanic Terms," by M. C. Cooke, publishers, London; Hardwicke & Bogue, price 2s. 6d. This little work is eminently useful for reference.—Amateur Botanist.

[317]—Seal Fisheries.—Seals are skinned (in a few minutes), their carcases are left (alive) on the ice until the cold or death puts an end to their sufferings. If allowed space, I will give you the details of a day in Greenland. Some call it sport, but I have another name for it.—T. D. Rennie.

[320]—Botany.—If "F" requires an inexpensive work, I should recommend him to procure "Lindley's School Botany." It is a capital book, giving an insight into "Structural Botany." If he intends making a collection of wild flowers, he will find "John's Flowers of the Field" (profusely illustrated) an extremely useful book.—J. C. L.

[326]—Dorsetshire "Valley Terraces."—These are the ramparts of the ancient "camps," or fortified towns, for which Dorset is remarkable. Several are very prominent objects from the railway.—R. N. Worth, F.G.S.

[329]—Phosphorescence of Fish.—It seems most probable that this phenomenon is due to the process of putrefaction, the decomposition by oxidation of the bones, of which phosphorus is an important constituent. It is observed in the decay of all animal substances, but more especially in the case of fish; so that in the instance of the cured haddock, chemical action would take place in a sufficient degree to liberate the phosphorus in small quantities, forming P_2O_3 with the O of the air. From recent investigations, it has been set forth (I believe by M. Chappuis) that phosphorescence is due to the generation of ozone.—R. C. F.

[332]—Mosses.—Let "Eupteris" try Wilson's "Bryologia Britannica;" it is an excellent work on British mosses.—Alexander Blake.

[333]—Sakkara Tablet.—This list of the Kings of Egypt was discovered by the late M. Mariette about 1863, and is now in the Boolak Museum. It was found in the tomb of a priest named Tounari, who is represented rendering homage in the name of Rameses II., in whose reign he lived, to fifty-eight of the monarch's ancestors. Among these are a large number of names, especially of the first six, and extending to the nineteenth dynasties. This invaluable list was supplemented in 1864 by the so-called second Tablet of Abydos, also found by Mariette, which, with the sixty-four names of kings engraved on the "Hall of Ancestors," from Karnak, at Paris, and the first Abydos tablet in the British Museum, made up the monumental lists of the kings. M. Maspero's arrangement of these is to be found in his "Hist. Ancienne des Peuples de L'Orient," published by Hachette, of King William-street. Any recent history of Egypt will give an account of the tablet, as also does Lenormant in his "Ancient History of the East," published by Asher; for a complete commentary see De Rougé "Monuments des six Premières Dynasties.—A Member of the Society of Biblical Archæology.

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* Knowledge, *the increasing circulation of which compels us to go to press early in the week.*

Hints to Correspondents.—1. *No questions asking for scientific information can be answered through the post. 2. Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries. 3. No queries or replies savouring of the nature of advertisements can be inserted. 4. Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge. 5. Correspondents should write on one side only of the paper, and put drawings on a separate leaf. 6. Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

Studens. Our edition of Ganot later than yours, and cannot identify the passage. However, if $n(\gamma+\delta)=\alpha+\beta+\gamma+\delta$, it follows that

$$(n-1)\ (\gamma+\delta)=\alpha+\beta$$

How you "eliminate $(\gamma+\delta)$ from both sides" passes my comprehension. If you have 40 marbles and 40 tops, and are told they are equal in value to 3 shillings and 6 pence + one marble and one

top, how are you going to eliminate the marbles and tops from the statement? Do you not at once learn from it that 39 marbles and 39 tops are worth 3s. 6d.?—BOREAS. (1.) Axial rotation of earth constant all the year round. (2.) No evolutionist supposes there were germs of life in the earth during its incandescent condition.—C. J. C., S. C. SARGISSON, PHILO, SYMPATHY, C. H. OLIVER, G. F. S. CALL, DISPENSER, G. GORDON, H. W. Z., PETER PARLEY, J. HARKINSON, G. W. R., M. N., L. LUCAS, and *hosts* of others, have answered queries about "Burial of Moses," Ferrier's Influenza Powder, and plants in bedroom.—H. A. BULLEY. (1) We are not, so far as I know, approaching another glacial period. (2) Inclination of earth's axis remains almost unchanged. (3) Present condition in sense that earth is nearest to sun in winter of northern hemisphere, will be reversed some eleven thousand years hence; but whether that would be favourable to glacial northern epoch or not, is open to question.—ASHBRIDGE H. ROBINSON. Certainly *not* for reason named that Mr. Grant left out date of Stone Age period. Have you seen any, even the slightest trace of unwillingness on our part to put facts as we find them, because they do not square with preconceived ideas (whether relating to facts, fancies, or faiths)? Fifty thousand years ago, excentricity of earth's orbit was less than now (0.0109; now 0.0168), longitude of perihelion 28° 36', so that earth was nearest to sun towards end of October. That was not the glacial epoch, or time of Stone Age. For a time when excentricity was very great take 850,000 B.C., excentricity 0·0747 (!), and longitude of perihelion 239° 28'—earth nearest to sun towards end of August. Very glacial time for northern hemisphere then.—DR. LITTON FORBES. Many thanks; a most interesting subject.—J. L. SINCLAIR. Theory that satellites were expelled from primaries by explosive force will not bear examination.—E. C. It will make no difference which way you put the lens.—W. J. L. (1) Ophite is a synonym of serpentine, but also sometimes applied to speckled green porphyry. (2) Mr. Brough was librarian at the London Institution, successor of Brayley in the office, and predecessor of Mr. Nicholson, now Bodleian librarian. He was a well-known writer on scientific, chiefly chemical, subjects.—F. H. S. About the eye—the experiment proves nothing as to inversion of image. We know from optical laws that the image must be inverted. The other questions too wide,—such queries remind me of the lady who, just before supper, at a ball, said to Humboldt: "Now we have five minutes to ourselves, *do* tell me all you know about the earth." Sir R. Phillip's idea sheer nonsense.—GEO. E. NEWTON. Thanks for the pamphlet against vaccination. I do not wonder that no notice has been taken of it by Dr. Carpenter. Ill-written, ill-reasoned, and offensive, how does it deserve notice?—J. MCDOWELL. Probably Sir Henry Thompson meant the term vegetables to include fruit. Can one say that it does not? What are tomatoes, pumpkins, vegetable marrows, and so forth? Consider again the lines—

> Amidst them stood the tree of life,
> High eminent, blooming ambrosial fruit
> Of vegetable gold.

Does not this seem to imply that Milton at any rate thought fruit a vegetable production? Science thinks so, too. You would "define a vegetarian as one who excludes from his articles of diet flesh, fish, and fowl;" surely strict vegetarians would exclude eggs, milk, butter, cheese, and so forth. These are distinctly animal food. Then again, do you not rather cavil about words than facts, when you dwell on his remark about debarring ourselves from the *right* to use such and such food? "Surely he knows," you say, "that vegetarians do not debar themselves from the right," &c. Might he not reply that, in holding it to be wrong to use vegetable food, they *do* debar themselves from the right to use it? It seems to me that Sir H. Thompson's letter was eminently fair and judicious, and conceived entirely in the spirit of his own doctrine, that it is unwise to conclude over hastily that what suits ourselves must necessarily be the best for others. If your letter had not been so very long, it would have appeared.—R. H. I doubt if any reader could tell you of a way by which you could successfully make a Rainband spectroscope.—LIVERPOOL. The zodiacal light is certainly more than the mere radiance from the sun after he has set. It is very seldom distinctly seen in our latitudes. When seen (in February or March in the evening, and in the autumn in the morning), it appears as a slant cone, the apex being some 80° or 90° from the sun's place. It is incorrectly depicted by Dr. Phipson, in his excellent little work on meteors, as a vertical cone. It is best seen an hour or two after sunset.—F. PRATT. You will pardon my remarking I did not "permit the planets to be described here as the cause of weather changes." I distinctly said that I must draw the line somewhere, and I drew it just there.—E. P. C. Geologists estimate the thickness of strata (without digging directly down through the depths they name), by noting the thickness of successive layers, as they crop up to the surface, and adding these thicknesses together. I do not myself think that the moon's mass was thrown off in a single effort, so to speak, thinking it more probable that the moon was first formed of multitudinous small bodies separately thrown off, like spray, from the top of the great tidal wave. I think the rings of Saturn represent this earliest or embryonic stage of the moon's history.—S. HALL. The star you refer to just below "the little Bear" is Vega. You will be able to find the place of the planets now visible to the naked eye, from the zodiacal map in Part III., except Saturn, which you will find about as far to the right of Jupiter as Mars is to the left.—A. M. S. Queries noted.—THE VILLAGE PHILOSOPHER. One of the ways you mention must certainly be used to find the cube root, unless you resort to logarithms.—A. G. S. Your cat story is rather long; besides, the cat which follows the sweeper may "wish to gain some private ends."—N. Have already mentioned that query about noise in trains was lost in postal transit.—CONSTANS. Thanks.—ALEX. AITKEN. The only book I know of that would suit you is "Chauvenet," in two volumes. It is expensive, and rather difficult. For the other purpose you cannot do better than use "Guillemin."—GRADATIM. Shall probably write on the subject of space.—DANIEL JACKSON. I believe no small part of the success of KNOWLEDGE has been due to just those sections (Whist and Chess) which you wish to see removed. If your own editorial experience had been half as satisfactory as mine, you would understand my unwillingness to make any changes. The contributor to whom you refer is well able to tell me himself, if dissatisfied. As he has not, I cannot but think your reference to what he said when at Hamilton a breach of confidence. Is mischief-making one of the devices which your editorial experience suggests as desirable for extending the circulation of a magazine? On the whole, I am inclined to believe I know how to conduct this paper at least as well as you can teach me.—JOS. BAXENDELL. I did not insert your reply to Mr. Ranyard, because it seemed written under the impression that he meant to offend you personally, which I am sure was not the case. Your remarks to myself had better not be inserted, because readers might imagine they were meant to offend me personally, which I am sure is not the case. Still, they are digressive—if not aggressive. I shall be glad to publish any arguments you may wish to advance. In the meantime, I do not see how you can deny that if there are five equations in each of which cosmical elements appear on one side, while one and the same number 1881·59 appears on the other, there can be formed no less than ten equations (the number of combinations of five things two together) having cosmical elements on both sides—giving, therefore, ten instances of remarkable but quite extra-pyramidal coincidences. You say you do not know what "Fudging" means, and that a young schoolboy friend, whom you have asked, cannot tell you. Perhaps in the North the term is not used. It means humouring numbers or facts so as to get some result you want. For instance, the answer to a sum should be, let us say, £1. 13s. 4d., and a schoolboy gets £1. 13s. 1d.: well, he puts the right answer in the proper place, and alters several of the preceding steps to correspond, taking his chance that the teacher will only glance over the few first steps and the few last, not finding out either the original error, or the place where it has been corrected by an artificial error. That is one form of fudging. For other instances, take your quiet assumption that the sun's real diameter has a certain value fitting in with your other results, or your other quiet assumption that the number 1881·59, which comes out among cosmical relations, is the exact length of the Great Gallery.—WM. WILSON. Well, since you ask me, I *do* think your letter in singularly bad taste I "need not" (need I not?) "trouble to reply." No trouble whatever, my dear sir; as you assume the office of censor, I venture, following suit, to tell you that, while there is no irreverence at all in what I said (quoting Shakspeare), there is gross irreverence in the way in which you, to all intents and purposes, attribute to the Almighty the susceptibilities of a soured pedagogue. *Dat veniam corvo* (W. W.) *vexat censura columbam* (R. A. P.).—C. F. B. HARSTON. Had not heard that "cats lie on their heads, with one or both ears on the ground, when it is going to rain." It may be the case; but I wonder how it is done. The "sun entering a sign" is quite different to the sun being in a "house"; the former relates to the sun's passage along the ecliptic, the latter to his position in those imaginary divisions of the heavens with reference to the horizon on which astronomers base their predictions. When astrologers said a person was born under a particular planet, they meant that that planet was in the ascendant (the part of the heavens about to rise above the horizon) at the time of the person's birth.—C. Do you call the series $(2\times\frac{2}{1})+(4\times\frac{3}{2})+(6\times\frac{4}{3})+(8\times\frac{5}{4})+(10\times\frac{6}{5})+(12\times\frac{7}{6})$, geometrical? The definition of a geometrical series is sufficiently precise, and certainly does not include such series. Surely "Teacher" is justified in his criticisms.—X. Surely you are not in earnest in asserting that the oar is not a lever of the second kind. If the object of the oarsman was to push the water along, the oar would be a lever of the

XUM

first kind, but as his object is to move the boat, and as the boat's weight is urged from the rowlock, the rowlock cannot possibly be regarded as the fulcrum; for the fulcrum in every lever is the *point d'appui* from which the weight is lifted.—Erin. The dates and hours mentioned beside the monthly star map, mark the times at which the stars are in the position shown in the map. The map, of course, can be used at any time within an hour or so before and after the times named. Your suggestion noticed; you will find we are not at all inattentive to suggestions intended to enhance the usefulness of the maps.—Gradatim. We know of no general solutions for such problems as: "Find two consecutive integers <100, which shall contain no factors other than powers of 2, or 3, or 5," &c. Such questions are scarcely suited for our columns.—E. D. G. Queries about plants in bedroom, scientific terms, and salt already sufficiently answered.—F. J. Stevenson. We intend soon having some papers on the Arabic names of stars.—One Not Convinced. (1.) Have you ever tried to bring a plumb-line into line with a star, from a distance of (say) 200 yards? You will find it easier to talk of than to do. (2.) A pie-dish full of water would have reflected Alpha Draconis as well as a body of water in the plugged-up hole; so also a passage an inch in diameter would have shown the star as well as the four-feet-square descending passage,—*if* you chanced to get it in the right direction. The difficulty is to do it. By taking only a small reflecting surface you produce precisely the same effect as if you diminished to corresponding degree the cross-section of the descending passage. With water poured in, as I have suggested, the ascending passage would be equivalent optically to the prolongation of the descending one. 3. You "cannot debit such a waste of material as the grand gallery, used for so short a time, to men otherwise so clever as the Pyramid builders." How are you going to avoid the difficulty? It applies far more to Smyth's theory. You might have waited till my theory was fully described. The question you asked over name "Bumberum" has been asked, and, by the way, has not been answered. A spinning peg-top presents many difficulties.—M. H. C. Thanks; but that was not what Newcomb meant. He has admitted the erroneous nature of his explanation.—Jas. Crain. Thanks for magic cube; will put it by in the hope that ere long I may use it.—Jas. Grundy. Many thanks.—L. W. H. When you look at a landscape in an inverted position (best not in public) a part of the retina receives the image which in the ordinary attitude is not used and is therefore more sensitive.—Ecliptic. There is no general formula by which magnitude of eclipse and times of beginning and ending for any place can be computed.—W. M. Science cannot yet explain the qualities which the ether of space seems to possess. Read Herschel's Essay on Light (Familiar Science Lectures).—W. A. C. Mr. Hutton does not assert, but admits that were animals not used for food many must starve. He is intensely humanitarian, some think extravagantly so. Of every class of animals, including man, many times as many are brought forth as could possibly co-exist. Some *must* perish.—D'Artaguan. It is a matter of opinion. It seems to me unnecessarily confusing to describe same number repeated several times as an arithmetical and also a geometrical series; and to ask what proportions of gold, silver, and copper, exist in an alloy, when the equations show that there is no copper at all. The purpose of examinations is to ascertain what the examined know, not to perplex them.—W. F. See our monthly papers, beginning with No. 21.—J. S. P. Paper on the Transit of Venus soon. Have written no work on Practical Astronomy.—E. Greatorex. We scarcely take such matters as part of our regular subjects. For general chemistry provision already made. Other questions answered.—K. N., Lepus, J. Minor, C. A. E., Ripple, Alphard, Knowledge, G. P. Benstead, Carryl, S. S. S. S., Myrven, F. M., Amateur Reader, T. Boyer, D. A. N., Lomax, Boy, P. F. J., Scientia cum Legibus, Novice. Questions either too vague, or trivial, or unsuitable, or already discussed, or for other reason inadmissible.—Anti-Taranaki. Thanks.—W. S. Yes to both questions.—Theo. James. Already answered.—G. H. Mortimer wants G. E. V. to tell him how to slip the bobbin of wire over magnet, and what size wire is.—C. C. C. Newton's theory of comets' tails would only account for a certain definite rate of emission, just as known density of our air and known force of gravity gives a certain calculable rate for rising of a balloon in still air. Comets' tails are formed at a much greater rate.—C. Harrap. Questions have been referred to botanist. Cannot find space for replies so lengthy.—W. H. Harland. Thanks, but toads in holes now disposed of, and space crowded.—J. C. L., T. J. Woodrow, and others. Your stories are interesting, but, unfortunately, many subscribers consider enough said for present about intelligence in animals.—E. Cox. Observe the influence of your protest.—J. C. L. There *has* been a good deal of it.—More Light. Will see about it.—R. Humphreys. Writer of articles on Brain Troubles not acquainted with the hymn tune "Midian;" the addition to the echo chords he devised himself.—Omicron. What is Rhyolite?—R. S. Standen. Trembling of fixed stars due to "movements" in our air, not moisture, as misprinted, p. 412. Planets do not tremble, because they have discs; stars are appreciably the merest points.—Howard Williams. Letter forwarded to printers. No, you did not mention our former acquaintance, and I therefore inferred you were only a namesake of the H. W. I knew at St. John's.—Thos. Mactaggart. Thanks, but no space.—J. Gibson. Oh, but excuse me; the squares of $+a$ and $-a$ *are* equal.—Cornwall. Thanks; query referred to electrician.—H. A. N., J. M., Arthur Buckheim, Brenton, Kit, and others. What is the logarithm of a negative quantity? The logarithm of a number to a given base is that power to which the base must be raised to equal the number. To what power must any given positive number be raised to make it negative? It seems scarcely worth while to discuss a paradox thus arising from misuse of a function invented for a special purpose.—C. Harris. Paradoxes well known.—J. McGrigor Allan. If you could only put your objections against vegetarianism into smaller space.—W. H. Wood. Question answered.—Phosphor. Germ theory not inconsistent with evolution. One of your other queries inserted. When one correspondent sends six queries at once we begin to think of closing the Queries column.—E. M. (Cantab). When the theory has been established it will be time to consider how it was originated.—Hallyards. Axis of Venus does not point towards sun. No; I think you saw what you thought you saw, but that what you saw was not what you thought it. You are quite mistaken in attributing the origin of the usage you mention to Knowledge. It has been customary for years, in press, pulpit, and lecture-room, though of course not customary with all. It has been adopted in my own case repeatedly. Your paper about jelly fish is in type. Will you excuse me if I hint that you have evidently much more leisure than I have. If one correspondent in twenty wrote at such length, or if our circulation increased twenty-fold, as we hope it will, what could a poor editor do? Replies to such letters as yours would in that case fill all our space.—M. B. Alder. Pardon me. I by no means welcome Dr. Siemens' theory. I have given Dr. Carpenter's report of it; but it seems to me (I venture to say, *it is*) utterly untenable. There is no such centrifugal tendency as he imagines, and suns cannot at same time do the work he describes and shine through interstellar space as they do.—M. Wyatt. You don't explain why you take 22 to the power 5, and not 21 or 23, or some other number between 20 and 30.—Zares. You have attacked too difficult a subject.—Herbert Puckle. Thanks; but we would rather not encourage such kindness. Your book might not be returned, and we should feel (though you, no doubt, would not hold us) responsible.—W. H. Pigrum. Theory too vague.—W. Smith. It would be much more convenient if two of the 31-day months gave up one day each to February. But in such matters the human race is very slow to change.—John Carter. I was thinking rather of cases where the training from the beginning had come into the teacher's hands; in other words, of cases in which a parent had been the teacher. I can well believe there are cases where bad home training leaves the teacher little choice but to use some form of corporal punishment.—E. V. H. The indications of the spectroscope are reliable to a certain point; they do not tell us everything, however. Spectroscopic evidence respecting larger comet presently.—Cabinet Maker. Cannot give addresses of correspondents.—Samuel Steettard. There was a transit of Mercury in Nov. 11, 1861, and one in Nov. 5, 1868; none eighteen years ago. You could not have seen an intra-Mercurial planet in transit, if there were one. Most probably, what you saw was a spot, and though "apparently the size of a five-shilling piece" (how far off?) was, probably, considerably larger.—H. H. Harris. There are many cases in which the old poets made their words resemble in sound what they were describing. I know of none much more striking than this, in a description of frying:—

Quis non—norit
Stridentesque focis opsonia plebis alausas.

—N. The illusion is practically the same which Mr. Foster has described and illustrated in No. 1. However, it shall appear.—Henry Carr. It is science which is exactly worded and plainly described, not the magazine. The point you notice was carefully considered in full conclave.—John Hamer. Thanks for kindly letter. As for your explanation, we wait.

Pond's Extract is a certain cure for Rheumatism and Gout.
Pond's Extract is a certain cure for Hæmorrhoids.
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Notes on Art and Science.

DOLLOND'S SIDEREAL WATCH.—Almost every amateur astronomer requires to know approximately, if not exactly, the sidereal time. He can calculate it, of course, from ordinary time, for any given instant, but the difference between sidereal and ordinary time is always changing, so that the calculation made at one time will not avail at another. On the other hand, not every astronomer can afford an instrument so costly as a sidereal chronometer. Mr. Dollond, the well-known optician, has devised a neat, simple, and very useful sidereal watch—such an instrument as every amateur astronomer should carry in his pocket.

PROFESSOR PASTEUR'S PREVENTIVE INOCULATIONS OF CHARBON.—The Prussian Minister of Agriculture, the *Deutsche Med. Woch.* (Feb. 11) states, has appointed an influential scientific committee to superintend and report upon a series of inoculations to be performed by one of Pasteur's assistants. This gentleman then proceeds to Russia for the same purpose, and on his return to Saxon Prussia, where the experiments are to be made, will perform a second series of inoculations. Besides some celebrated veterinary professors, Professor Virchow is expected to take part in the inquiry; but regret has been expressed that Professor Koch, the able critic of Pasteur, has not been nominated.

PLANTS IN BEDROOMS.—Plants are unhealthy in bedrooms for this reason, that during the night they give out carbonic bi-oxide, which, as is well known, is injurious to life. Plants, like animals, are constantly breathing—taking in oxygen, and giving out carbonic dioxide. During the day-time they feed as well as breathe, one of their chief articles of diet being the very same poisonous gas which they are constantly expiring. This carbonic di-oxide, under the influence of sunlight, and by means of the colouring matter (chlorophyl) is separated, the carbon being assimilated, and the oxygen evolved. In the daytime there is more oxygen given off than carbonic di-oxide, so that plants may be said to be healthy in the light, but unhealthy in the dark. I may add that the quantity of either gas given off in a room from a few plants is so small as to be hardly worth noticing.—F. D. H.

Our Mathematical Column.

MOGUL'S PROBLEM.

THE problem being "Given any rectangle, divide it by the fewest possible straight cuts, so that the parts can be put together to form a square," my solution is as follows:—

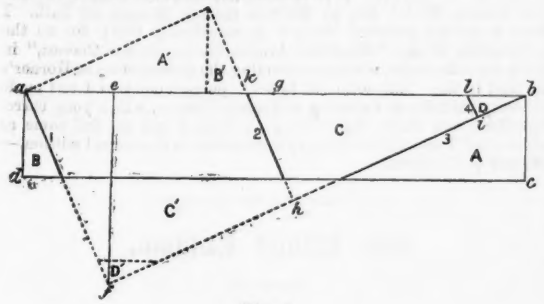
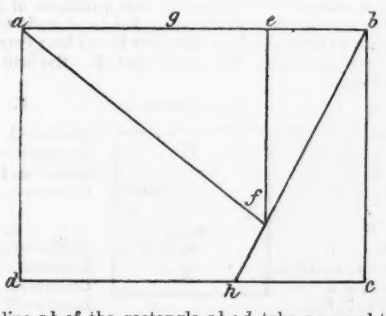
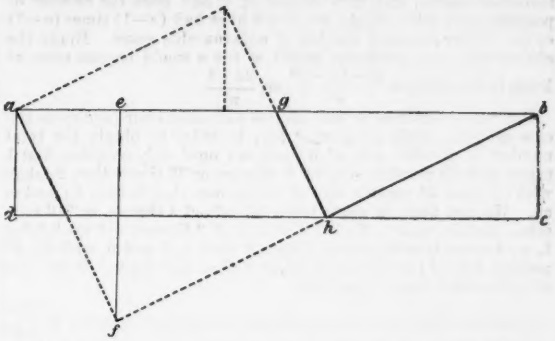


Fig. 1.



On the line $a\,b$ of the rectangle $a\,b\,c\,d$ take $a\,e$ equal to $a\,d$, and make $e\,f$ perpendicular to $a\,b$; bisect $a\,b$ at g, and, with the radius $a\,g$ and centre g, describe a circle cutting $e\,f$ at f, join $a\,f$ and $b\,f$, and make $f\,h$ and $h\,i$ equal to $a\,f$; draw $h\,k$ and $i\,l$ parallel to $a\,f$.

By cutting the rectangle at such parts of the lines $a\,f$, $k\,h$, $l\,i$, and $f\,b$ as pass through it, you will obtain pieces which will form the desired square, *vide* Fig. I., in which rectangle, $a\,b$ is six times $a\,d$. The principle, however, will be the same, whatever may be the proportion between the sides; but in cases where the proportion does not exceed two to one only two cuts will be necessary; not exceeding five to one, three cuts; not exceeding ten to one, four cuts; not exceeding seventeen to one, five cuts; and so on.



It will be observed that my method of finding the side of a square whose contents are equal to that of a given rectangle, is different to that given by Euclid, II., 14. Calling the sides of the rectangle x and y respectively, Euclid's method is equivalent to the mathematical proposition that—

$$xy = \left[\frac{x+y}{2}\right]^2 - \left[\frac{x-y}{2}\right]$$

whereas my method is equivalent to the proposition—

$$xy = \left[\frac{x}{2}\right]^2 + y^2 - \left[\frac{x}{2} - y\right]^2.$$

It may puzzle some of your readers to discover how my method involves this last equation.—MOGUL.

[Correct solutions by R. Horne (two, both very neat), P. E. M., H. W. Partial solutions by H. J., N., E. Whitby, and others. Solutions by G. H. Bonner, and H. Jones incorrect.—ED.]

PROBABILITIES.

The true method in dealing with problems of the kind considered in our last, is to reduce them to the general law first established by determining—(1) *How many possible events there are;* (2) Whether these are all equally likely; and (3) how many are favourable.

Our question is: What is the chance of throwing one Ace at least in two trials with a single die? Now, when such a die is thrown twice, the following are the possible throws:—

1, 1	2, 1	3, 1	4, 1	5, 1	6, 1
1, 2	2, 2	3, 2	4, 2	5, 2	6, 2
1, 3	2, 3	3, 3	4, 3	5, 3	6, 3
1, 4	2, 4	3, 4	4, 4	5, 4	6, 4
1, 5	2, 5	3, 5	4, 5	5, 5	6, 5
1, 6	2, 6	3, 6	4, 6	5, 6	6, 6

The table being formed by combining first throw 1, with any one of the second throws 1, 2, 3, . . . 6; first throw 2 with any of the same set of 6; and so on. The total number is 36, or 6 times 6. Any pair in *the first column*, or in *the top line*, gives at least one Ace—that is, there are 11 favourable pairs out of 36 possible pairs. Also, it is obvious that any pair of the 36 is as likely to be thrown as any other. Hence, by what was shown in paper I., the chance of throwing Ace at least once in two casts of a single die is $\frac{11}{36}$. The chance of failing is $\frac{25}{36}$. It will be noticed that the number of unfavourable cases is 5 times 5, the total number of cases being 6 times 6. It is clear that a table containing all the unfavourable cases would be formed in precisely the same way as the above table; and that, in fact, such a table is actually included in the above table, omitting the upper line and the left-hand column.

Now the way in which the above result is obtained would be inconvenient in practice. Suppose, for instance, that instead of a die

we had a teetotum with twenty faces, numbered 1, 2, 3, up to 20. Then the table formed on the plan shown above would take along time in the writing. But it is only necessary to notice how the above table is formed, to obtain a simple general rule for all such cases. We have in six successive columns the six numbers—1, 2, 3—6, each repeated six times, in order that they may appear in company with the same six numbers, 1, 2, 3—6, so that the total number of pairs is 6 times 6. And, obviously, if any number, as n, replaced 6, we should get n times n, or n^2, as the total number of pairs. In like manner, we have in the above table, 5 times 5 unfavourable cases; and if n instead of 6 had been the number of possible cases taken singly, we should have had $(n-1)$ times $(n-1)$ or $(n-1)^2$ for the total number of unfavourable cases. Hence the chance that any particular result of the n would happen once, at least, in two trials is $\frac{n^2-(n-1)^2}{n^2}$, or, $\frac{2n-1}{n^2}$

But now to return to our die, we can extend our inquiry to the case of three trials or more. For, in order to obtain the total number of possible sets of 3 rows, we need only conceive first 1 taken with all possible sets of 2 throws, or 36 times, then 2 taken with all those 36 possible sets of two throws, then 3, then 4; and so on. We get thus, in all, 6 times 36 sets of 3 throws, or 216 such sets. And in order to find all the sets of 3 throws, not containing 1, we have only to take first 2, then 3, then 4, 5, and 6, with the 25 possible sets of two throws in which 1 does not appear at all, and this gives us 5 times 25, or 125.

Hence the chance that ace will not be thrown in 3 trials is $\frac{125}{216}$; and the chance that it will be thrown once at least in three trials is $\frac{216-125}{216}$ or $\frac{91}{216}$.

And clearly we have this general rule. If there are the same n possible events in each of three trials, the chance that some particular event will not occur in any of the three trials is $\frac{(n-1)^3}{n^3}$, and the chance that it will occur once at least in one of the three trials is $\frac{n^3-(n-1)^3}{n^3}$. And by proceeding in this way we get the yet more general rule:—If there are the same n possible events in each of r trials, the chance that some particular event will not occur in any of the r trials is $\frac{(n-1)^r}{n^r}$; and the chance that it will occur once at least in the r trials is $\frac{n^r-(n-1)^r}{n^r}$.

This general result is of extreme importance, as we shall presently see. It is of importance, not only in inferring the antecedent probability, as to the result of successive trials, where the conditions of each trial are known, but also in inferring from the results of successive trials (or observations or experiments, if we please) the conditions (supposed unknown) under which those trials have been made.

I close this paper with two simple examples of the application of this rule.

1. *What is the least number of trials which would give a person at least an equal chance of throwing ace with a single die?*

We have seen that his chance of failing to throw an ace in two trials is $\frac{25}{36}$, and in three trials $\frac{125}{216}$. In four trials the chance of failing will be $\frac{625}{1296}$. Now we note that $\frac{125}{216}$ is greater than a half, and $\frac{625}{1296}$ is less than a half. Hence in three trials he is more likely to fail than to succeed, and in four trials he is more likely to succeed than to fail. Therefore four is the number of trials required. In any continued series of sets of three trials, he would fail somewhat oftener than he would succeed, in throwing one ace at least. But in any continued series of sets of four trials, he would succeed oftener than he would fail.

2. *What is the chance that, in three tossings of a coin, head will appear once at least?*

In each trial there are two possible events, *i.e.*, the n of our rule is equal to 2. Thus the chance that head will not be tossed in three trials $\frac{1^3}{2^3}$ or $\frac{1}{8}$. Therefore the chance that head will appear once at least in three tossings is $\frac{7}{8}$. The odds are 7 to 1 that one head at least will be tossed in three trials; and if there were to be repeated sets of three trials, a bettor backing the appearance of one head at least in each set should lay these odds. Further, if a person is to receive £8 in case a head appears in three trials, he ought to pay £7 for his chance.

MATHEMATICAL QUERIES.

[39]—Chances,—Required—

1. The chance of dealer (at Whist) holding only one honour in any particular suit.
2. The chance of the dealer holding at least one honour in any particular suit.
3. The chance of the dealer holding only one honour in Trumps.
4. The chance of the dealer holding at least one honour in Trumps.

Gradatim.

["Gradatim" sends solutions of these problems.—Ed.]

[40]—What is the general solution of the equation

$$\frac{d^2\theta}{dt^2}+a\left(\frac{d\theta}{dt}\right)^2+b\theta=0$$

and show how it is obtained.—Wilhelm.

ANSWERS TO QUERIES.

[27]—*The value of a diamond varies as the square of the weight. A diamond is broken into three parts: determine the probable value of the parts, compared with that of the unbroken diamond.*

Let x, y, and $a-(x+y)$ be the weights of the parts into which the diamond is broken, a being the weight of the whole diamond, and $\therefore x^2+y^2+(a-x+y)^2$=sum of value of parts. To find the mean average value, assume that x remains constant, and that y has every value between 0 and $(a-x)$; add up the value of the above expression; and divide by the number of values. That is, integrate the above expression, with reference to y, from 0 to $a-x$, and divide by $a-x$. Hence we get

$$\frac{x^2(a-x)+\frac{(a-x)^3}{3}+\frac{(a-x)^3}{3}}{a-x}=\frac{3x^2+2(a-x)^2}{3}$$

Give to x every possible value from 0 to a, and divide by the number of values. That is, integrate the above expression from 0 to a, and divide by a. Hence, we get—

$$\frac{a^3+\frac{2a^3}{3}}{3a}=\frac{5}{9}a^2$$

Hence, the required proportion is $\frac{5}{9}$ T. R.

Horner's Method.—In reference to the method of extracting "All Roots," given in "Our Mathematical Column" at p. 440, allow me to observe that it was not discovered by "the late Mr. Horner, M.P.," but by William George Horner, of Bath. I have a special personal interest in mentioning this; for on the publication of my "Practical Arithmetic for Senior Classes," in 1858, the *Athenæum*, while commending the introduction of Horner's method in the "Extraction of Roots," pointed out that I had made the very mistake of referring to Francis Horner, which your correspondent now does. On writing to Bath, I got the full name as above, and I made the necessary corrections in the second edition.—Henry G. C. Smith.

Our Whist Column.

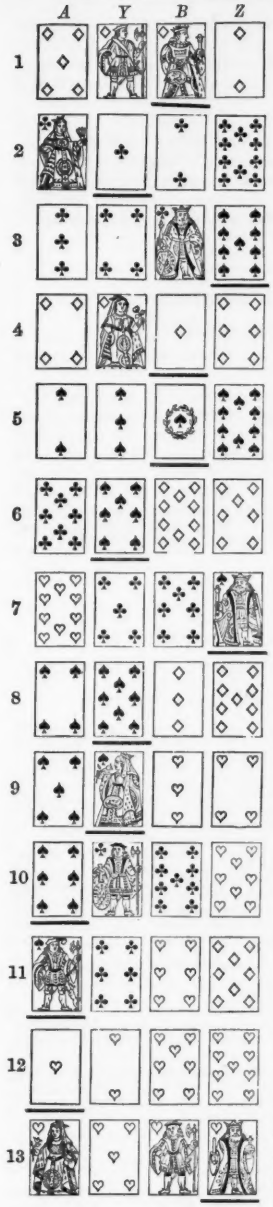
THE following game, from the Westminster papers for August, 1874, is selected as illustrating the weakness of lead from short suit; even when the odd trick only has to be made to win, and the idea is not to bring in long suit, but to use long trump suit to ruff. Our correspondent, Mr. Lewis, was *Y*. His lead at trick 7 is worth noticing.

A.	The Hands.	*Y.*
Spades—Kn, 6, 5, 4, 2. Diamonds—5, 4. Clubs—Q, 8, 3. Hearts—A, Q, 10.	B Y Dealer. Z *Trump Card, Spade King.* A	*Spades*—Q, 8, 7, 3. Diamonds—Q, Kn. Clubs—A, Kn, 6, 5, 4. Hearts—5, 2.
B.		*Z.*
Spades—A. Diamonds—A, K, 10, 3. Clubs—K, 9, 7, 2. Hearts—Kn, 7, 6, 3.		*Spades*—K, 10, 9. Diamonds—9, 8, 7, 6, 2. Clubs—10. Hearts—K, 9, 8, 4.

Score.—*A B*, 4; *Y Z*, 4.

THE PLAY.

NOTE.—The underlined card wins trick, and card below it leads next.



REMARKS AND INFERENCES.

1.—*A*, with five trumps, one honour, leads from his shortest suit, hoping to play a ruffing game. His hopes, it will be seen, are not fulfilled by the event.

2.—*Y* having five Clubs, and seeing lowest Club led, which shows that *B* is not leading from short suit, can pretty safely infer that *Z* has played his only Club. Being strong in trumps himself he returns his opponent's lead (which is from his own long suit), forcing *Z*.

3.—*Z* being short in trumps, would ruff even if the trick were a doubtful one.

4.—*Z* like *Y* returns his opponent's lead. Having five Diamonds, and noting that neither the three nor the four fell to trick one, he can infer, with some degree of probability, that *A* has led from a short suit, in which (from his play) *Y* is also short. Trick 4 shows exactly how the case lies, and *Z* can place every Diamond. *Y* also sees how the Diamonds lie.

5.—*B*, having the winning Diamond, takes out a round of trumps before leading it, knowing his partner's play, and that *Y* lying over him, *A*'s plan is not likely to prove very successful.

6.—*Y* ruffs, of course, though holding four trumps, and

7.—Leads his lowest Club to draw his partner's trump card. He can count the Clubs, knowing that *B* must originally have held four, and he knows, therefore, that if he leads the best, *A* will trump. By playing the lowest, he causes his partner's King to fall separately. The odd trick and the game are won *at this point*.

8.—*Z* leads the best diamond, knowing his partner to lie over *A*. It matters not how *A* plays as the cards lie, but, "for the sake of uniformity," (*Y* having already renounced), *A* should have played his best trump or none.

10.—*Y*, finding all trumps left with *A*, throws the lead into his hand, knowing that he must lead a Heart either after or before last trump, and that the trick wanting to win the game must in that way be secured, unless *A* and *B* between them have entire command of Hearts, in which case the game is gone anyhow.

PROBLEM II.

Trick 1. *A* leads Spade Ace.
2. *A* leads Club Ace, trumped by *B*.
3. *B* leads small Spade, trumped by *A*.
4. *A* leads King of Clubs, trumped by *B*.
5. *B* leads Spade, trumped by *A*.
6. *A* leads Queen of Clubs, trumped by *B*.
7. *B* leads Spade, trumped by *A*.

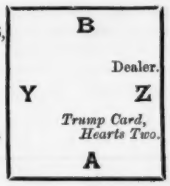
In Problem 3, hands *A* and *B* were inadvertently transposed. [The fault was mine, not "Five of Clubs."—ED.] However, it is so obvious that *A* cannot win every trick, as the hands are set, that we suppose no Double Dummy problem solver has been for a moment deceived. Teddington, J. K. L., R. Morrison, and F. X. Y. have correctly solved the problem, all of them, however, first transposing the hands of *Y* and *Z*, which does not quite make the problem right, though, as it chances, not affecting the solution. The problem is a pretty one, and we now give it correctly, and shall leave it for a fortnight for solution.

PROBLEM III.—Double-Dummy.

THE HANDS.

A.
Hearts—Kn, 6.
Clubs—5, 3, 2.
Diamonds—A, Q, Kn, 6, 5.
Spades—A, Q, Kn.

B.
Hearts—A, Q, 10, 9, 4, 3.
Clubs—10, 6.
Diamonds—3.
Spades—10, 9, 8, 7.



Y.
Hearts—K, 8, 7.
Clubs—9, 8, 7, 4.
Diamonds—2.
Spades—6, 5, 4, 3, 2.

Z.
Hearts—5, 2.
Clubs—A, K, Q, Kn.
Diamonds—K, 10, 9, 8, 7, 4.
Spades—K.

The lead being with *A*, *A* and *B* make every trick.

E. F. B. Harston. Yes: line 30 from top, first column, p. 462, for Ace and Queen read Ace and King. The correction was obvious. Problem 2 is sound. In problem 3, a transposition has to be made. When you say that player at Double Dummy has made every trick, do you necessarily imply that he made them all out of his own hand? In ordinary Whist a player would say, "We have made every trick," when every trick falls between his own hand and partner's, but in Double Dummy he would hardly say that.—FIVE OF CLUBS.

Our Chess Column.

GAMES BY CORRESPONDENCE.—(*Continued from p.* 461.)

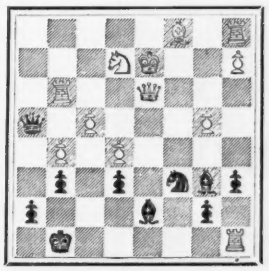
Black's 23rd move in Game I. was notQ to R4, but Q to R5. With that rectification the following are the positions corrected from last week :—

GAME I.

Position after Black's 23rd move.

23. Q to R5.

CHIEF EDITOR.
WHITE.



BLACK.

CHESS EDITOR.

WHITE.	BLACK.
24. B to R3	R to Qsq
25. R to R3	Q to B7
26. R to B3	Q to Kt7
27. R to Kt3	Q to B7
28. R to B7	Q to Kt7

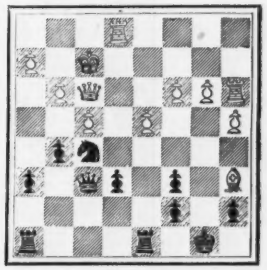
Drawn game (perpetual move).

GAME II.

Position after White's 22nd move.

22. Q to KB3.

CHIEF EDITOR.
WHITE.



BLACK.

CHESS EDITOR.

WHITE.	BLACK.
22.	R to Q3
23. R to K5	K to Rsq
24. R to Kt5	R to Q Ktsq
25. R takes R(ch)	K takes R
26. R to R2	P takes P
27. R to K2	Kt to K6
28. P takes P	Kt to Q4
29. P to KB5	

Had Chess Editor played as indicated by mistake to Chief Editor, viz., 23. Q to R4; 24. B to R3; 24. R to Qsq; 25. Q to KB3, he might have proceeded as follows :—25. Q takes Q; 26. R takes Q; 26. Kt to Q5. With best play White may draw.

For if (a) 27. $\frac{\text{Kt takes Kt}}{\text{B to B3}}$ 28. $\frac{\text{R to Kt3 best}}{\text{B takes Kt}}$ 29. $\frac{\text{R to KBsq}}{\text{B to Q4}}$ 30. $\frac{\text{R to Ktsq}}{\text{B takes RP}}$ threatening B to Kt7 disc. ch. and to win the Bishop. The variations arising from this line of play are highly interesting, but we find that the two Bishops aided by the Rook get the best of the struggle in every case. White's best course would

be to give up the exchange voluntarily, and then a draw might result.

(b) If, in reply to 26. Kt to Q5, White does not play Kt takes Kt, but 27. R to R3, then follows—

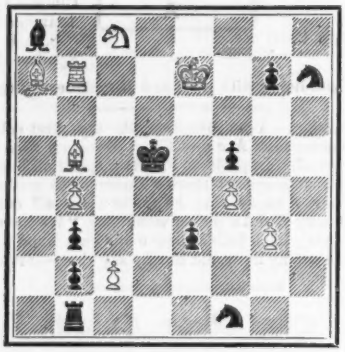
27. B to B3 — 28. Kt takes Kt / B takes Kt — 29. R to QKtsq / B to KKt7

and again, by proper play, the Bishop will force the exchange. In addition, Black might follow another line of play, viz., by discovered check and other judicious play he could capture the Pawns on White King's side, and then Black's Pawns would become formidable.

PROBLEM No. 30.

By J. A. Miles.

Black.



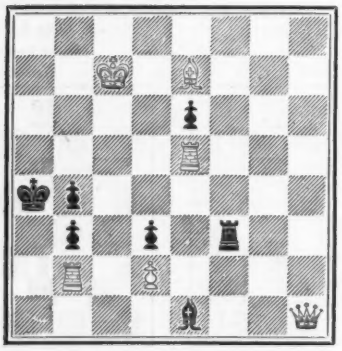
White.

White to play, and mate in three moves.

PROBLEM No. 31.

By Leonard P. Rees.

Black.



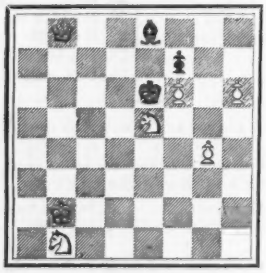
White.

White to play, and mate in two moves.

PROBLEM No. 32.

By B. G. Laws.*

Black.



White.

White to play, and mate in three moves.

* Published anonymously in the *Boys' Magazine*.

CORRECTION.—Problem No. 25, p. 441.

Remove Black Pawn from R3.

Our numerous correspondents who have kindly drawn our attention to this problem, please take the above correction (which already appeared last week) in consideration. We shall be happy to receive the correct solution of this problem, which embodies a very neat idea.

A great international Chess Tournament will be held at Vienna, beginning on May 10. Prizes to the amount of £400 will be given. The first prize will be £200. Messrs. Steinitz, Blackburne, and Zukertort, the three best players of the whole world, will compete. This Tournament promises to eclipse anything hitherto attempted in Chess Tournaments.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

J. A. Miles.—Thanks for Problem, which has an improved appearance.

Leonard P. Rees.—We apologise for mistake in your Problem No. 25. Problem and Game welcome. In the last variation of the "Giuoco Piano," p. 442, if White does not play 10 Kt to B3, threatening to capture the Black Knight, but 10 Castles, then Black need not reply with 10 B to Kt5; a likely line of play for Black in reply to 10 Castles would be Castling, to be followed by P to KB4. White's centre Pawns always remain weak, and liable to attack and isolation by P to QB4. Whichever way you play, White is on his defence, instead of having the attack in hand as first player.

W. Wood.—Received with thanks.

Muzio *v.* H. Grinold.

A. H. E., W. W. Morgan, 23, Great Queen-street.

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